

N. 5



Philip Carl. Kurfürst



AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL
TRANSACTIONS,

GIVING SOME

A C C O U N T

O F T H E

Present Undertakings, Studies, *and* Labours,

O F T H E

I N G E N I O U S,

I N M A N Y

Considerable Parts of the W O R L D.

VOL. XLIV. PART II. For the Year 1747.

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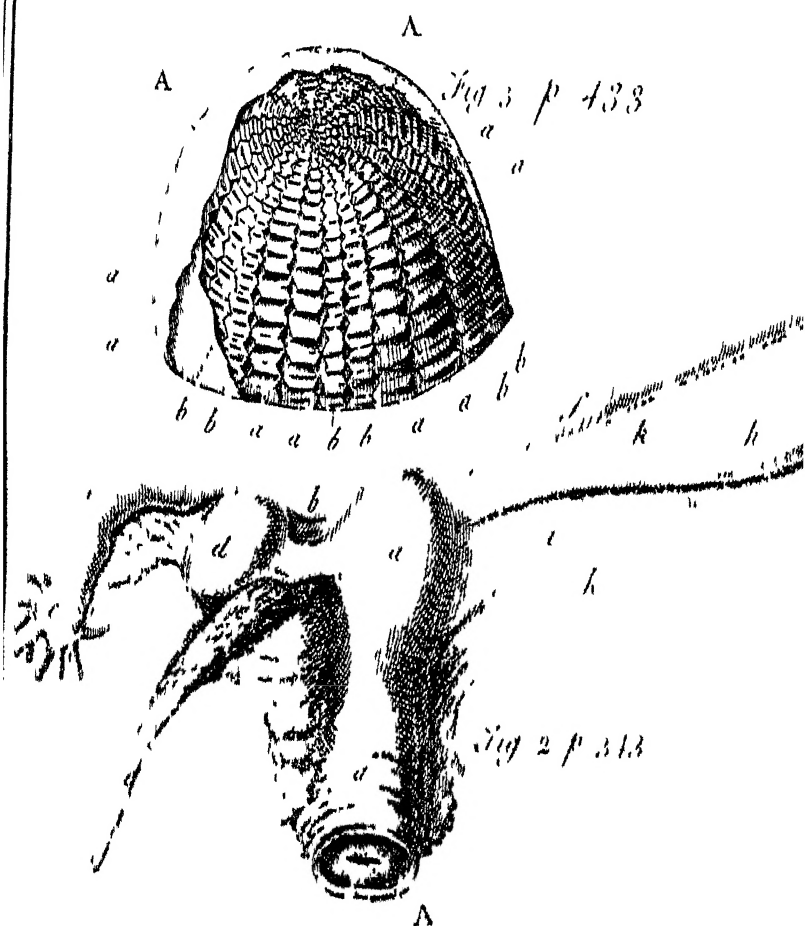
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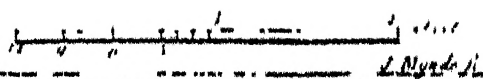
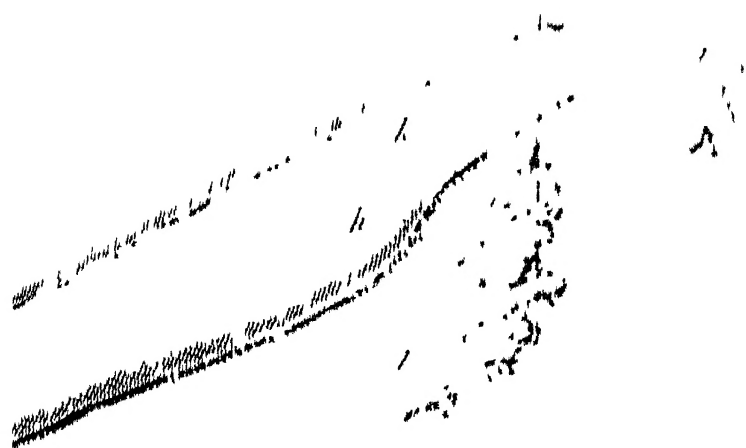
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PHILOSOPHICAL TRANSACTIONS.

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ERRATA.

p. 299. l. 9. for $\frac{1}{10}$ read $\frac{4}{10}$. for p. 2.

- I. *A Letter from Mr. Wm. Fidge, Surgeon, at Portsmouth, to Cromwell Mortimer, M. D. Secret. R. S. concerning a Stone taken out of the Bladder of a Dog; which being cut asunder had a Piece of Dog-Grass in its Center.*

S I R,

Read at a Meeting of the Royal Society, Jan. 8. 1746-7.

ACCORDING to your Directions I have sent an Account of the Stone I left with you; It is now fourteen Years since I took it out of the Bladder of a very large Mastiff, about five Years old, belonging to the Porter of his Majesty's Dock-Yard at *Portsmouth*. The Dog died in about three Days after receiving a Kick from some one endeavouring to part him from another Mastiff he was fighting with.

Being then an Apprentice to the Surgeon of the Yard, and hearing the Dog was dead, I sent the Labourer, who attended the Surgery, to get him for me, in order to dissect.

When I had open'd the *Abdomen*, I found it fill'd with bloody Urine; and having before heard that his Death was suppos'd to be occasion'd by the Kick, I immediately thought the Bladder must be the Part hurt; which, when I had cleansed the *Abdomen*, I examined, and found this large Stone, with the Bladder contracted close to it on every Side, and rent at the Bottom about three Quarters of an Inch; so that what Urine came to the Bladder was discharg'd

U n

into

into the *Abdomen*; which was plainly the Cause of his Death.

When I first took it out it weigh'd 10 Ounces 2 Drachms and an half. It is not more than two Months since I cut it asunder; when, finding it form'd upon (as I imagine it it is) a Piece of Dog Grass, I thought it would not be an unacceptable Present to the Curious; therefore, having some Affairs which call'd me to Town, I brought it with me for that Purpose.

What is to be farther remarked is, that I did not find any the least Particle of Gravel or Sand either in the Kidneys or Ureters; and that all the Bones (except the Ribs and *Cranium*) are more or less carious, as the Bones you have with the Stone.

If, when you shew it to the *Royal Society*, it should be thought deserving a Place among their Curiosities, it will give me the greatest Pleasure, in having this Opportunity of presenting something worth the Notice of so Learned and Ingenious a Body of Gentlemen. I am,

S I R,

With all due Respect,

Your most obedient,

London, Dec. 29.
1746.

humble Servant,

William Fidge.

II. *An uncommon Dropfy from the Want of a Kidney; and a Description of a large Saccus that contain'd the Water, by Samuel Glafs, Surgeon, at Oxford; sent to Dr. Mead.*

November 11, 1746.

Read Jan. 8.
1746-7.

MART Nix, who lived at *Hampton-Poyle*, a small Village in *Oxfordshire*, had been remarkable all her Life for the preternatural Size of her Belly. After her Death, I had the Curiosity, together with some learned Gentlemen of the University, to inspect her Body. Her Mother was then present, and inform'd us, that this her Daughter was born dropfical; that she herself had been ill of the same Disease for some time before, and during, her Pregnancy; but, on the Birth of this Child, she was freed from that Disorder.

The Child, tho' born dropfical, prov'd otherwise healthy; and, notwithstanding the Disease continually increased as she grew up, liv'd to be near twenty-three Years of Age.

She was a tall well-proportion'd Woman, except with Regard to the enormous Size of her Belly; and, for one of so unwieldy a Bulk, healthy, brisk, and active. Her Appetite was always good, and she was never more than ordinarily thirsty; had no remarkable Difficulty of Breathing, not even when she lay supine, nor did her Thighs or Legs ever swell. Her *Menses*, which appear'd at the usual Time of Life, continued regular, till within eight Months of her Death. The only Complaint was now-and-then a

Pain in making Water; and the Quantity she made was commonly about four or five Ounces.

Upon the Suppression of her *Catamenia*, there succeeded a *Dyspnœa*, Loss of Appetite, Emaciation of the superior Parts, and a Tumefaction of one of her Legs with Ulcerations. These Symptoms gradually increased till her Death.

Upon taking the Dimensions of her Body before Dissection, we found the Circumference of her *Abdomen* to be just six Feet four Inches, and from the *Xyphoid* Cartilage to the *Os Pubis* it measur'd four Feet and half an Inch. The cutaneous Vessels, distributed upon the *Abdomen*, were remarkably large, and distended with Blood, and the spurious Ribs were pressed greatly outwards and upwards.

After this general View of the external Parts, we began the Dissection, by dividing the Cartilages of the six superior Ribs, and raising the *Sternum*. The *Thorax* being laid open, we observed that the Diaphragm was forcibly protruded into that Cavity. The Base of the Heart lay under the right Clavicle, and its *Apex* upon the most convex Part of the Diaphragm; which Convexity advanced as high up as the third superior Rib. The Lungs were surprisngly small, scarce exceeding in Magnitude those of a new-born Child. The right Lobe slightly adher'd to the *Pleura*, the left was free, and both were in a sound State. Within the *Pericardium* was found, as usual, a small Quantity of Liquor, but none in the Cavity of the *Thorax*.

We next perforated the *Abdomen* in the most convenient depending Part, and evacuated from thence a surprisng Quantity of Water, which was lightly tinged

tinged of a Coffee-Colour, limpid as Urine, and not in the least ferid. This Water was carefully measur'd, and found to be not above a Pint less than thirty Gallons Wine Measure; which must weigh, according to the common Calculation, near 240 l.

We afterwards made an Incision into the *Abdomen* along the *Linea alba*. The Integuments upon the epigastric Region were very thin; the abdominal Muscles much extenuated; and above the *Umbilicus* the *Tunica cellulosa* contain'd no Fat; but from the Navel to the *Os Pubis*, the *Panniculus adiposus* was half an Inch thick. Upon dilating the Incision, the large membranous Bag that contain'd the Water presented itself to View, adhering transversely about ten Inches to the anterior Part of the *Peritonæum*.

This Adhesion. being separated, we had a full View of this wonderful Reservoir, which was of an enormous Size, and had almost occupied the whole Cavity of the *Abdomen*. In Figure, Colour, Thickness, Number, Magnitude, and Distribution of Blood vessels, it very much resembled the *Uterus* of a Cow at the End of Gestation. The whole Inside was scabrous, and look'd as if parboil'd; and here and there was observ'd a small Quantity of a Coffee-colour'd Sediment. On the left inferior Part was discover'd the Orifice of a Duct, which open'd obliquely into the Cavity of the *Saccus*, and would easily admit of a large Goose-Quill. From this Opening the Tube advanc'd about twelve Inches between the Membranes of the Bag obliquely upwards, and towards the right, from whence it was inflected downwards, and pass'd between the Duplicature of
the

the *Ligamentum latum Uteri*, to be inserted into the Bladder of Urine. The *Saccus* was connected to the *Ligamentum suspensorium Hepatis*, to a considerable Part of the *Mesocolon*, to the *Peritonaeum* on the right Side in two or three different Places, to the same Membrane the whole Length of the Spine, and to the *Ligamentum latum Uteri* on the right Side of the Body.

The Liver was found, but less than in a natural State; and its convex Part adher'd closely to the Diaphragm. The Stomach, Spleen, *Omentum*, small Intestines, and the upper Part of the *Colon*, were thrust very high up into the left *Hypochondrium*. The Convolutions of the lower Part of the same Intestine were intirely obliterated; and that, together with the *Rectum*, formed one continued strait Tube, from the left *Hypochondrium* down to the *Anus*. The left Kidney, with its emulgent Vessels and *Ureter*, were in their natural State and Situation. The *Uterus*, *Tuba Fallopiana*, and *Ovarium*, on the same Side, had nothing preternatural; but, on the right Side, the *Fallopian* Tube and Ovary were dispos'd in a very extraordinary Manner. The Tube, by means of the Adhesion of the *Ligamentum latum Uteri* to the *Saccus*, was extended to three times its ordinary Length. The Ovary was likewise, by the same Cause, render'd very preternatural, being no less than five Inches three Quarters long, one Inch broad, two Tenths of an Inch thick, and two Inches and half distant from the *Uterus*. The Bladder of Urine was very small, but appear'd to be sound.

We then made an accurate Search for the right Kidney; but, to our great Surprize, found no such

such *Viscus*, nor any thing analogous to it, unless the *Saccus* that contain'd the Water already describ'd, may be esteem'd such: And what seem'd to favour this Opinion, was the Disposition of the emulgent Vessels on the right Side, which were propagated from the *Aorta* and *Vena cava* to this *Saccus*, in the same manner as to the Kidney on the opposite Side; and, after having ran twelve or fourteen Inches between the Membranes of the Bag without any Ramifications, were distributed all over it in the Manner before-mention'd.

From the foregoing Account the following Queries are naturally suggested, which I leave to the Determination of the Learned :

Query 1. Was not the *Saccus* originally a mis-shapen Kidney, and the Duct a *Ureter*?

Query 2. Was not the Water contained in the *Saccus* prevented from growing putrid, by being continually drain'd off thro' the Duct into the Bladder of Urine, and by being afresh supplied by the emulgent Artery; and more being secreted than was evacuated, the Quantity thereby continually increased?

Query 3. Was not this the Reason why the Patient had never any anasarcaous Swellings of her Thighs or Legs, nor any Thirst, or other Signs of a confirm'd Dropsy?

Query 4. Were not the Lungs prevented from growing by the great Diminution of the Cavity of the *Thorax*, and the Pressure they sustain'd from the distended *Abdomen*? And might not their never having occupied a larger Space than they did at Birth, be the Reason she never labour'd under any Difficulty of Breathing?

Query

Query 5. Was not the Bladder of Urine likewise by the superincumbent Weight, prevented from dilating itself; and that the Reason why the Water was often made, and always in so small a Quantity?

In order to convey a more clear Idea of the several Parts already describ'd, to those who may not have an Opportunity of inspecting them (they being properly preserved for that Purpose), and being sensible of the great Difficulty of clearly representing by Words such Things as are out of the ordinary Course of Nature, I must beg Leave to refer the Reader to the Figures hereto annex'd.

See TAB. I.

FIG. I.

- aaaa*, The great *Saccus* that contain'd the Water.
- bb*, The greater Diameter of the *Saccus*.
- cc*, The lesser Diameter.
- d*, A prick'd Line shews the Entrance of the emulgent Vessels.
- ef*, Shews the Course of the Duct between the Membranes of the *Saccus*.
- f*, The Orifice opening obliquely into the Cavity.
- eg*, The inflected Part of the Duct, after its Egress from the *Saccus*, which passed between the *Ligamentum latum Uteri* to the Bladder of Urine.
- g*, The Duct, divided near the Bladder, where its Cavity was so small as only to admit of a common-siz'd Probe.

FIG.

FIG. 2.

Represents the posterior View of the *Uterus*, *Fallopian* Tubes, and Ovary.

aa, The *Uterus*.

A, The *Os Tineæ*.

bc, The *Fallopian* Tube in a natural State

c, The *Morsus Diaboli*.

d, The left Ovary in a natural State.

ee, The *Ligamentum rotundum*.

fg, The right *Fallopian* Tube.

g, The *Morsus Diaboli*.

hh, The right Ovary.

ii, The *Ligamentum rotundum*.

kkkk, The *Ligamentum latum Uteri*.

ll, Its Adhesion to the *Saccus*.

III. *An Explanation of an ancient Inscription discovered at Rutchester, the last Station in England, upon the Roman Wall, 1744 ; by John Taylor, LL.D. Chancellor of Lincoln, and Register of the University of Cambridge.*

Read Jan. 15.
1746-7

	IMP. CAES. M. AVRELIO.
	SEVERO. ANTONINO.
AV.	PIO. FELICI. M. PARTHIC.
i. e.	MAX. BRIT. MAX. GERM.
<i>Augusto.</i>	MAX. PONTIFICI. MAXIM.
	TRIB. POTEST. XVIII. IMP. II.
	COS. IIII. PROCOS. P. P. COH. I.
	FIDA. VARDVL. CREO \otimes ANO
	<u>NNANA. FECIT. SVB. CVRA. TCO</u>
	LEG. XX. GR.

According to the Copy given me by Dr. *Hunter* of *Durham*, who copied the Inscription this last Summer.

This Inscription addressed to *Caracalla* has nothing in it very singular, except the Title of the Cohort that dedicated it, namely,

FIDA. VARDVL. CREO \otimes ANONNANA.

concerning which I shall offer these few Conjectures.
The

The *Varduli* were a People of *Hispania citerior*, mentioned by *Pomponius Mela* and others; and are recorded now, in no less than three Inscriptions, to have served in *Britain* as Auxiliaries. The two other are printed in *Horsley's Britannia Romana, Northumb.* N°. xciv^d. *Durh.* xxvi. We find Troops of several Nations to have been here upon the same Occasion, as *Cohors prima, secunda, &c. Batavorum, Dacorum, Nerviorum, Tungrorum, Delmatarum, Thracum, &c.*

This Cohort of the *Varduli* is intituled FIDA, a very common Appellation, and moreover CREO⊗ANONNANA; the last Letters of which I separate, and read without inserting a single Letter ANTONINIANA, thus, ANONNANA. Nothing is so frequent in Inscriptions, as this compendiary Way of writing ANTONINVS, and its Derivatives. Thus in *Horsley's* Inscriptions,

M.AVR.ANTOÑNO.PIO.

North. cxiii.

M.AVRELI.ANTOINI.PII.FELIC.

North. xciv^d.

COH.III.II.BR.

ATONNIA.

North. lxxvi.

Upon this Hint therefore I am persuaded, that when the Stone is next inspected, these little *Apices* will appear, which are easily overlook'd, when this brief Manner of Writing is not expected or attended to.

In regard to the Appellation *Antoniniana*, it is observable of the ancient Militia, that several of their Cohorts and Legions, as well *Roman* as Provincial, complimented themselves with the Imperial Surname, of which I shall produce some Instances.

DIS. MANIBVS.
C. ANTONI. C. F.
OVF. AEGRILLI.
MIL. LEG. VII. CLAVDIAE.
PIAE. FELIC. ET. FIDEL.
VIX. ANN. XXXIIX.
M. VII. D. VIII.
M. AEGRILIUS. FRATRI
DVLCISSIMO. F. C.

Gruter. DXXI. 4,

I. O. M.
COH. I. AEL.
DA. GORD
.ANA.

Horst. Cumberl. VIII.

i. e.

Imperatoris Optimo Maximo COHORTIS *Imperatoris* AELIAE DACORUM
GORDIANA.

NVMINI FOR
TIS. FORTVNE.
M. AVRELIVS.
MARINVS. MIL.
COH. VII. PR. P. V.
SEVERIANAE.

Gruter. MXIII. 10.

i. e.

COHORTIS VII^{ma} PRÆTORIAE PIAE VITRICIS SEVE-
RIANAE. But

But the Imperial Addition ANTONINIANA is perhaps the most frequent of any, as the Name of *Antoninus* was assumed by a long Series of Emperors.

DEAE. EPONAE. M. OPILIVS. RESTIO.
MILES. LEG. XXII. ANTONINIANAE. P. P. F.

Gruter. LXXXVII. 4.

H. D. D.
DEABVS. MATR:
IVLIVS. REGVLVS. MI
LES. LEGIONIS. VI.
ANTONINIANE.

Gruter. XCII. 2.

D. M.
MARINA. SECVN
DINA. VIX. ANN. LIII,
L. AVRELIA. SATVR.
VIX. ANN. XXI.
L. AVRELIVS. INGENVVS.
MILES. LEG. XIII. GEM.
ANTONINIANAE.
MATRI. ET. SO
RORI. POSVIT.

Gruter. DXXVIII. 3.

D.....

ET. MEMORIAE.
AETERNAE. VE
GETINIAE. RO
MANAE. MEM
MIVS. RVSTICVS.
MILES. LEG. V.
VICTRICIS. AN
TONINIAN. CON
IVGI. INNOCENTISSI
MAE. LOCO. PERE
GRINO. DEFVNCT.
P. C. ET. SVB. ASCIA. D.

Reinesf. VIII. 68.

But more particularly the two following, as they both bear Date under *Caracalla*; and one of them the very Year of his fourth Consulate, when he was collegued with *Balbinus*, and of consequence nearly coæval with ours. This is to be seen in *Gruter*, XLIV. 2. and runs thus:

P. AEL. VICTORINVS. ROMANI. VIVENI. F.
AVRELIANA. POPA. MIL COH. V. PR. ANTONINIA
NE. P.V.7.VERI. HERCVLI. DONV.POSVIT.LIBI
ENS.VOTO.VT.GRATIAS.AGO.DEDICAVIT.

III. NON. NOVEMBRES.

IMP. ANTONINO. AVG. N. IIII. ET. D.
CAELIO. BALBINO. II. COS.

The other is somewhat earlier, and bears Date between his second and third Consulate.

IMP.

IMP. CAES. M. AVRELIO.
 ANTONINO. AVG. SEVERI. F.
 TRIB. POT. V IIII. COS. II.
 COH. I. VIG. ANTONINIANA.
 CN. RUSTICVS. RVFINVS. PRAEF. VIG.
 TIB. CLAVDIVS. MARCELLINVS. TRIB.
etc.

Reinesf. III. ult.

There is yet a third, which, besides the Title *Antoniniana*, and the Correspondency of Date (for it was erected in the Consulate we have been speaking of) is likewise remarkable for the same *Scriptura compendiaria*. I shall give it whole from *Gruter*, CVIII. I.

IN. H.	D. D.
GENO. SANC	
TO. M. AVREL.	
CL. POMPEIAN	
VS. MIL LEG. VIII.	
ANTONINAE	
AVG. B. F. COS. K.	
IANVAR. IMP. D. N.	
ANTONINO. IIII. ET.	
BALBINO. II. COS.	

What remains to be accounted for is CREO⊕. Which Letters if any one should compare with *Horfley's Durham* Infcription, N°. xxvi. where mention is made of the same *Varduli*, he will find a very strong Resemblance ;

Resemblance; and be apt to conclude, that what explains the one, will bid very fair to explain the other. That Inscription runs thus,

. O. M.
 ATI. COH
 VARDVLOR.
 CREQ∞
 V.S.L.L.M.

CR then I take to be distinct Marks, and expressive of *Civium Romanorum*. And of this I find little Room to doubt, when I observe the same Marks applied to several Corps, who were as strictly Provincial as our *Varduli*; such as *Afri*, *Asturientes*, &c.

L.PRAESENTIO.L.FIL.
 LEM.PAETO.
 L.ATTIO.SEVERO.
 PRAEF.COH.Ī.AFR.
 C.R.EQ.IVDICI.SELECTO.EX.
 V̄.DEC.PR.AVXIMI.PAT.COL.
 AEDILI.ĪI.VIR.ANCONAE.

Gruter, CDLIX. 9.

SEXTL.CLASENO
 PRAEF.COH.
 Ī Ī Ī.ASTVR.EQ.C.R. etc.

Gruter. CCCLXXXVII I. 3.

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DIANAÆ
SACRAVIT
T.FL.
ITALICVS
PRAEF.
ALAE.I.VLP.
CONTAR.
∞.C.R.

Gruter. XL. 3.

DEIS DEABVS.
ALA.I VLP.CONT.
∞. C.R.CVI.PRE.
Q. ER. INGENVVS.

Gruter. II. 6.

But still more exprefly,

..... CEREALI.
...LEG. III. GALLICAE
... III. COH. GALLICAE.
civIVM.ROMANORVM.

Gruter. CDXCIX. 3.

And moreover what kind of Citizens, we are told
more than once; *viz. Juris Italici.*

T.FL.BARSI.V
ETER.ALAE.I.FL.
AVG.BRIT.
∞.C.R.IVRIS.ITALICI.
MEMOR. FR
ATRI.SVO.POSVIT.

Gruter. DXLI. 8.

Y y

T.

T.F.VERECVND.
 MAG.EQVVS.ALAE.
 I.FLA.AVG.BRIT. ∞.
 C.R.IVR.ITALICI.

Gruter. DXLII. 7.

The Freedom of the City had been for some time before this a regular Reward for the Fidelity of the Provinces, or any other military or civil Merit. The famous Oration of the Emperor *Claudius*, or the Act for incorporating the People of *Vienne* in *Gaul* (a large Fragment of which is preserved in *Gruter*, p. DII.) is a remarkable Instance of what I have advanced. QUID. ERGO. NON. ITALICVS. SENATOR. PROVINCIALI. POTIOR EST. IAM. VOBIS. CVM. HANC. PARTEM. CENSURAE. MEAE. APPROBARE. COEPRO. QUID. DE. EA. RE. SENTIAM. REBVS. OSTENDAM. SED. NE PROVINCIALES. QUIDEM SI. MODO. ORNARE. CVRIAM. POTERINT. REITIENDOS. PVTO. For so the last Words must be emended, as they have been restored by *Reinesius* and *Grævius*.

Afterwards, probably a little before the Date of our Inscription, which is near the End of the Reign of *Caracalla*, came the general Constitution of that Emperor; the Memory of which being fresh might probably occasion the Insertion of the Words *Civium Romanorum* in this Monument. *In orbe Romano qui sunt, ex Constitutione Imperatoris Antonini Cives Romani effecti sunt*, are the Words of *Ulpian*, in the first Book of the *Digest*. *Tit. de Statu Hominum*, Law XVII. That the *Antoninus* there mention'd was our *Caracalla*, is abundantly made good by Baron *Span-*
hem,

hem, in his Comment upon that Text. To which let me add the Words of *Prudentius*, produced by the same learned Writer upon the Occasion.

*Hinc frenaturus rabiem Deus, undique gentes
Inclinare caput docuit sub gentibus isdem,
Romanosque omnes fieri, quos Rhenus et Ister,
Quos Tagus aurifluus, quos magnus inundat
Hiberus.*

*Corniger Hesperidum quos interlabitur, et quos
Ganges alit, tepidique lavant septem ostia Nili.
Jus fecit commune pares, et nomine eodem
Nexuit, et domitos fraterna in vincla redegit.*

What remains EO⊗, I would restore EQ⊗, according to the Model of the *Durham* Inscription produced above :

COH. VARDVL. C. R. EQ. ∞ .

For the Difference in Stones is so minute in this Particular, that I will venture to pronounce, tho' I never saw the Inscription, it may as well be one as the other. And I read the whole Title of the Cohort thus :

COHors Ima FIDA VARDVLorum Civium Romanorum EQuitate
Milliaria ANTONINIANA.

Concerning which I have something to offer.

The frequent mention of Equestrian Cohorts, or, to speak more adequately, of *Cohortes EQuitate*, in old Inscriptions, I observe to have been a great

Choque upon several Antiquaries, who have been taught to consider the Cohorts as appropriated to the Foot Service, as the *Alæ* and *Turmæ* were to the Horse. Mr. *Horsley* in particular, p. 94. imagines, the *Cohors prima Claudia Equitata*, which he met with in the *Notitia*, was intended to intimate, that this Cohort had been promoted from the Foot to the Horse Service. But when that Gentleman was led, by the Mark or Monogram in the *Durham* Inscription refer'd to in these Papers, to consider that Corps as consisting of a thousand Horse, his Difficulty is increased to that Degree that he does not know what to affirm upon it. Now of all this there is a very easy Solution.

The Auxiliary or Provincial Cohorts (for of them only I observe what follows) were either intirely or purely Foot, like the Legionary and ordinary Cohorts; or else they had a Mixture of both Kinds of Militia, and consisted of Horse and Foot together.

IMP. CAESAR. DIVI. VESPASIANI. F. DOMITIA
NVS. AVGVSTVS. GERMANICVS. PONTIFEX. MAXI
MVS. TRIBVNIC. POTES. XII. IMP. XXI. COS. XVI.
CENSOR. PERPETVVS. P. P.
PEDITIBVS. ET. EQVITIBVS. QVI. MILITANT. IN. COHO
RTE. III. ALPINORVM. etc.

Gruter. DLXXIV. 5.

This latter Sort, as they could not properly be rank'd under either Denomination of Horse or Foot, (for they were made up of both) seem to have appropriated to themselves the distinguishing Title of *COHORTES EQVITATAE*, Corps of Infantry with a Mixture

Mixture of Horfe. And of this Term, fo very significant, and fo little understood, I find frequent Mention.

L. BRVTIO. L. F.
PAL. CELERI.
EQVO. PVBLIC.
PRAEF. COH. III. AVG.
THRAC. EQVIT.

Gruter. DXXXIV. 2.

Q. CAECILIO. Q. F.
AN. OPTATINO.
PRAEF. COH. I. AQVI
TANOR. EQVIT.
PONTIA. T. F. SABI
NA. MATER.
FLAMINICA.
L. D. D. D.

Gruter. DXXXIV. 4.

P. LICINIO. P. F.
GAL. LICINIANO.
PRAEFECTO.
COHORTIS. VII.
PRAELECTORVM.
EQVITATE. IN.
GERMANIA.
TRIBVNO.
MILITVM. LEG.
VII.
EVIA. PRAEFECTO.

Gruter. DL. 4.

P. LICINIO. P. F.
GAL. MAXIMO.
PRAEFECTO.
COHORTIS. II.
GALLORVM.
EQVITATE. IN.
DACIA. TRIBVN.
MIL. LEG. VII.
CLAVDIAE. PIAE.
FIDELIS. P. LICINIVS.
LICINIANVS.
FRATRI.

Gruter. DL. 5.

M. MAENIO. C. F. COR. AGRIP
PAE. ET. VSIDIO. CAMPESTRI.
HOSPITI. DIVI. HADRIANI.
PATRIS. SENATORIS PRAEF. COH.
II. FL. BRITTON. EQVITAT.
ELECTO. A. DIVO. HADRIANO.
ET. MISSE. IN. EXPEDITIONEM. BRIT
TANNICAM. TRIB. COH. I. HISPA
NOR. EQVITAT. PRAE. ALAE. I.
GALLOR. ET. PANNONIOR. CATA
PHRACTAE. *etc.*

Reines. VI. 128.

Nor have we these Testimonies only, but also a full and decisive Proof of this Denomination, and, what is yet behind, of their Number also, in a Writer very well vers'd in military Affairs, *Hyginus*, who wrote

wrote a Treatise *de Castrametatione*, in the Time of Trajan. From him we are informed, that these Troops were call'd *Milliaria*, as consisting of a thousand private Men, Part Horse and Part Foot. The Proportion of the former of these to the latter was nearly as one to three, *viz.* 240 to 760, instead of 250 to 750. Which little Difference was possibly occasion'd by the Necessity of dividing them into *Centuria* and *Turma*. The Author's Words are:

Habet Cohors Equitata Milliaria pedites septingentos sexaginta, centurias decem, equites ducentos quadraginta, turmas decem.

And again;

Meminerimus itaque ad computationem cohortis equitatae milliariae pedaturam ad mille trecentos sexaginta dari debere.

Which is to be thus explained: The *Pedatura* of this irregular Corps, in their Incampment, would not be the same as the *Pedatura* of an uniform Body of Infantry, of the same Number, but would exceed it by 360 Feet; for the Proportion of the Room of one Horseman to one Foot-Soldier he assigns as $2\frac{1}{2}$ to one

Omnis miles provincialis accipit pedaturam, pedem, adjecta quinta — eques autem duos semis, adjecta quinta. — Habebit itaque Cohors equitata milliaria equites ducentos quadraginta, quos redigo ad peditem, ut pedem, quod accipit miles, ad duos semis, quod accepit eques. Fit dimidia sumpta ductum quinquies: sic tractabimus numerum equitum, fit centum viginti: ducemus quinquies, fiunt sexcenti:

sexcenti: accedunt ex eadem Cohorte milliaria, detractis equitibus, reliqui pedites septingenti sexaginta: fit cum superiori mille trecenti sexaginta.

For the Mark \oplus I account thus: The usual Note of a Thousand is either I between CC's, thus CIO; or else X, thus CXO. The former Figure, when closed at the Top, exactly resembles an ancient M, thus \ominus ; and the latter, when shut up, a Figure of Eight inclined ∞ . Both which Marks have been long used to express a Thousand. The latter is the Mark before us, the X between CCs, but closed in on all Sides, thus, \oplus , if this be in Reality the Figure upon the Stone. For Mr. *Gordon* in his *Iter Septentrionale* copying an Inscription, wherein there was the Mark of four Thousands, gave us the Thousand inclosed on all Sides, the very Mark in our Inscription; but upon Mr. *Horsley's* Inspection it turned out to be the second Figure, the Thousand inclos'd only at both Ends thus ∞ .

The last Part therefore of the Inscription is to be thus understood:

COHors I.^{ma} FIDA VARDVLorum Civium Romanorum Equitata Milliaria ANTONINIANA FECIT SVB CVRA T. CO..... Legati, Tribuni, or Centurionis LEGionis XX.^{ma} Genio Romæ.

Which last Words are to be applied to the Emperor, and contain a Compliment at that time of Day not unusual.

St. John's College, Cambridge.

Jan. 1. 1744-5.

IV. *An Abstract of the Rev. Mr. Gould's Account of English Ants ; in a Letter from the Rev Henry Miles, D.D. and F.R.S. to Mr. Henry Baker, F.R.S.*

Read Jan. 15. 1746-7. **I** HERE send you a short Abstract of an ingenious Treatise on *English Ants*, the Perusal of which has entertained and instructed me not a little ; and as the very industrious Author has made more Observations than any other Person amongst us appears to have done, and has discover'd several curious Particulars not mention'd by other Writers on the Subject, I thought you would not be displeased to see a brief Account of the Performance, with a few Remarks and Emendations I have taken the Liberty to make.

The Book is intituled, “ *An Account of English Ants* ;” which contains, 1. Their different Species and Mechanism ; 2. Their Manner of Government, and a Description of their several Queens : 3. The Production of their Eggs, and Process of the Young : 4. The incessant Labours of the Workers, or common Ants ; with many other Curiosities observable in these surprising Insects : By the Rev. *Wm. Gould, A.M. of Exeter-College, Oxon.* London : Printed for *A. Millar*, opposite *Katherine-Street* in the *Strand*, MDCCXLVII. in large 12mo.

CHAP. I. Contains a Description of Ants in general, their various Sorts, Colour, and Structure of their Parts.

Five Species of Ants have occurred to the Observation of our Author. 1. The Hill Ant, vulgarly called the Horse-Ant. 2. The Jet Ant. 3. The red Ant. 4. The common yellow Ant. 5. The small black Ant.

Having described the Size and Colour of these, he proceeds to describe the Structure and nice Mechanism of Ants with great Accuracy; observing, that, besides the *Viscera*, there is in the Body of Ants a Bag of corroding spirituous Liquor, which they can eject to a considerable Distance at Pleasure. This Particular has also been observed by other Writers.

He says, he has met with a Ligament in the red Ant, which uniteth the Breast and Body, consisting of two Lobes somewhat round; but in other Ants there appears but one Lobe, which rises higher, and is broader, than the Lobes in the red. It is this Species of red Ants, which he has observed to have a Sting, of the same Contexture with that of a Bee, in Miniature: In other Ants he has met with no Sting; but they bite, or make a small Incision, with their Saws, ejecting some of the afore-mention'd corroding Liquor, &c. The red Ants, which are furnished with a Sting, he observes live more open, &c. and are more bold than any of the others; and therefore such a Weapon is serviceable to them.

The Jet-Ants, he informs us, have a peculiar disagreeable Smell, which he imagines may be a great Preservative to them against an Enemy; — and that the Spirit which all Ants eject is very strong, affecting at a small Distance in the same manner as Spirits of Hartshorn.

CHAP. II. Treats of their Colonies, Cells, &c.

Here our Author observes, that tho' they unite in Colonies, in such Places and Situations as are most agreeable to their different Natures, &c. yet their Residence is not so limited as to admit no Variation; however 'tis worth observing, that the several Species never so intermix, as to associate and breed together, tho' they will live near and good Neighbours one to another.

Their Architecture, he says, is adjusted with remarkable Curiosity and Art, the whole Structure being divided into a Number and Variety of Cells, communicating all of them with one another by little subterraneous Channells, which are circular and smooth; but as for the Incrustation, most *Virtuosi* have mention'd, in the Apartments of Ants, our ingenious Author observes, that after the most careful Observation he could never find any Composition in their Structures; the Cells being formed in the Mold itself, without any Addition of Glew, Straws, &c. He acknowledges it may be otherwise in hotter Climates, where Sand is more apt to crumble.

Their Works, as he informs us, are all carried on by the Assistance of their double Saws, and the Hooks which are placed at the Extremity of them, described by him in the preceding Chapter. The Process and Manner of their Work may easily be observed, he says, if you deposit some Ants, with a Lump of moist Earth under a Glass.

CHAP. III. Treats of their Government; describes their several Queens; the Respect shewn them by the common Ants, &c.

A Colony, our Author tells us, from the latter End of *August* to the Beginning of *June*, is usually composed of a large Female, and various Companies of Workers. — And besides these, in the latter End of *June*, all *July*, and Part of *August*, of a Number of winged Ants commonly known by the Name of Ant-Flies. The Government, he says, has been universally taken for a Republic or Commonwealth; and have been treated as a Body consisting of Males and Females; the former being looked upon to be those which make their Appearance with Wings in the Summer. But as, in the Oeconomy of Bees, the Generality of them have no Distinction of Sex, but make it their whole Employment to provide for the Young laid them by their Queen, so the same Character is found to be maintained in the Constitution of Ants. The common Ants therefore, which usually present themselves to our View, are, he says, like the common Bees, of neither Sex, but seem intirely destined to take care of, and educate the Young, which the Queen deposits in the Cells.

In every perfect Colony, our Author says, there is at least one Queen; who, in the Space of 7 or 8 Months, gives Birth to a Family, amounting, at a moderate Computation, to 4 or 5000; except the red Queens, who are not so prolific. The yellow Ants being the most frequent, he gives a very particular and curious Description of their Queen; which, he tells us, is perhaps 5 times larger than any of her Subjects; and that, moreover, in her Front she has three Eyes, in a triangular Form, which are less than the two common ones on each Side her Head. I omit other Particulars, as also his Description of the other Queens,

Queens, for Brevity's sake. --- The Queen of the Jets, he says, he never had the Pleasure of seeing.

He has beautifully represented the Obedience and Respect the Queen commands, in whatever Apartment she condescends to be present. --- An universal Gladness, he says, spreads itself thro' the whole Cell, expressed by particular Acts of Joy and Exultation : They have a particular Way, it seems, of skipping, leaping, and standing upon their hind Legs, and prancing with the others ; which Frolicks they make use of both to congratulate each other when they meet, and to shew their Regard for the Queen. Some walk gently over her, others dance around her, and all endeavour to exert their Loyalty and Affection. However romantic, says our Author, this Description may seem, it may easily be proved, by placing a Queen, with her Retinue, under a Glass ; for, in a few Moments, you will be convinced of the Honour they pay, and Esteem they have for her.

In *October*, he tells us, Ants and their Queens begin to retire downwards ; and, in the Depth of Winter, are to be found in the remotest Apartments, incircled close with a Cluster of Attendants, and, as it were, benumb'd.

CHAP. IV. The Author gives a particular Account of the Time and Manner in which the Queens lay the Eggs, &c.

And he says, he has been the more circumstantial in this Point, to remove a Mistake of Sir *Edmond King's*, who, not aware of there being a superior Female, gave into the old Opinion, that
the

the small Ants were the Females, and supplied the Colony with Young: After a full Description of the Sperm or Eggs, Sir *Edmond* observes, that he found that Substance among the common Ants; and that he gave the more Credit to that Opinion, because of the great Care and Tenderness with which they treat it.---But our Author does not allow this Reason to be conclusive, inasmuch as the same is to be met with in the Constitution of Bees; adding, That having at all Times of the Year observed the common Ants, he could never discern any Alteration in their Bodies but what was occasion'd by Food, or some Accident.

The Queen, he says, lays three different Sorts of Eggs, Male, Female, and Neutral: The two first in the Spring; the last in *July*, and Part of *August*.

CHAP. V. Our Author treats of the Change of the Eggs to Vermicles, &c. and gives us an Account of their surprising Continuance in that State.

The Queen having furnished the Eggs, he says, the common Ants brood over them in little Clusters, perhaps by way of Incubation; and remove them to different Parts of the Colony, for the better Advantage of Moisture, and a just Degree of Heat and Cold. The Time of Continuance in the Egg-State is somewhat uncertain: But he says they seem to disengage themselves from the Membranes that inclose the Eggs in the same Manner as Silk-worms do.

The Process of Ant-Vermicles, he tells us, is remarkable, and worth Observation. The Female Eggs put on the Form of Worms some time in *February*, at farthest; the Male by the latter End of *March*;

March; the Neutral by *September*. The first Summer they grow very sparingly; the succeeding Winter they seem at a Stand: In the Beginning of *April* of the second Year they visibly augment every Day; and in six Weeks, or by the End of *May*, the Male and Female attain their greatest Proportions, and are ready for another Change. This long Continuance of Ants in a vermicular State he thinks a great Curiosity, hardly to be met with in any other Class of Insects---the Female Ant continuing above a Year and Quarter, the Workers a Twelvemonth, the Males somewhat more.

CHAP. VI. Treats of a Transmutation of Ant-Vermicles to Nymphs or *Aurelia's*, &c.

The Vermicles, he says, weave in the Manner of Silkworms, and in a few Days infold themselves in a soft filken kind of Tissue: They henceforth assume, and, whilst confined in this Monument, continue the Character of *Aurelia's*. These are the small Bodies which abound in the Settlements in the Summer-Months, and are vulgarly reputed Ant-Eggs; but their Largeness, and visible Transmutation (as he justly observes), shew the Mistake.

Our Author takes notice of a remarkable Variation in the *Aurelia's* of the red Ants. When the Worms arrive at their Period of Transmutation, he says, they do not infold themselves in a Tissue or Shell, like the others, but lie motionless, and, to outward Appearance, insensible; in a few Days look whiter than ordinary, and in this manner gradually put on the Form of Ants. Thus Providence (remarks our Author) is tied down
to

to no particular Laws : but can, by a surprising Variety, accomplish the same Ends.

In the VIIth Chap. he proceeds to treat of the Transformation of the several *Aurelia's* to Flies and common Ants, with a Description of their Structure, Duration, and other Curiosities relating to the Change. But the just Progress of Ants-Eggs, Vermicles, Nymphs, &c. cannot, he says, be precisely stated; because they will not arrive at Maturity under Glasses, as *Swammerdam*, before him, had observed.

As soon as the Ant-Nymphs, surrounded with a Tissue, are tending to Life, he says, the Workers give them Air, by an Aperture in the Head-Part of the Covering; which Aperture they gradually enlarge; and, after a Day or two, take out the Young, and expose it to the freer Access of the Sun-beams, which are of great Force in promoting its Maturity.

Our Author observes, that Philosophers have usually confounded the two different Sorts of Ant Flies, the large and small, looking upon them all under the Character of Males; tho' there be so wide and manifest a Variance in the Colour, Size, &c. that the naked Eye may easily distinguish it. — On the contrary, therefore, he presumes they are of different Sexes: The small ones he takes to be Males, and the large Females; and thinks it highly probable, that some of these Females, afterwards, give Birth to new Colonies, and intitle themselves to the Dignity of Queens; there being, as he says, many strong Experimental Reasons to support so uncommon a Curiosity; which he also recites, and answers the chief Objection against it, taken from the Number of these Ant-Flies: The principal Thing of which his Answer consists

consists is, that the most obvious Use of them is for the Sustenance of other Animals.

In the Close of this Chapter he annexes a few remarkable Curiosities resulting from the Change. --- The casting of their Wings is an Instance, he says, peculiar to the large Ant-Flies; these being to other Insects their highest Decorations; and the Want of them lessens their Beauty, and shortens their Lives. On the reverse, a large Ant-Fly gains by the Loss, and is afterwards promoted to a Throne, and drops those external Ornaments, as Emblems of too much Levity for a Sovereign.

CHAP. VIII. Our Author here treats of the incessant Labours of the Workers, the true Method of collecting their Provisions, and inquires into the Truth of the Opinion of laying up Corn, &c. against Winter, &c.

He says, The general Subject of this Chapter has been so largely treated of, and well illustrated, by some of the happiest Favourites of *Minerva* and *Apollo*, that it is impossible to set it off with more Beauty of Thought, or Elegance of Style; but perhaps, (says he) in many Circumstances they have rather shewn the Poet than the Philosopher; and rather indulged an extensive Fancy, than Strictness of Inquiry.

I must here omit the Account the Author gives of the Labour and Industry of the common Ants, which is certainly very curious, that I may avoid being tedious; observing only, in general, that the Feeding the Young is the most laborious Exercise be-

longing to the working Ants, and a Part of their Industry the most uninterrupted of any.

The Juices of most Sorts of Fruit, Insects, and Honey, or any other delicious Liquid, he says, are the Repast which they nurture them with. These Juices they extract, and first convey into their own *Alvins*, and afterwards infuse into the Bodies of the Vermicles; which Aliment, he supposes, may probably undergo some Refinement in the Repositories of the Ants, and, being there meliorated, is properly tempered for the delicate Structure of the Worms.

It has been a Dispute, says our Author, amongst the Inquisitive on this Subject, whether Ants have Magazines of Corn, and lay up a Stock of Provisions against Winter. The Generality of Writers, he says, hold the Affirmative; referring, in his Margin, to *Solomon, Pliny, Virgil, Horace, Aldrovand, Swammerdam, &c.* Here I am obliged to do Justice to *Swammerdam*; who, in his *Biblia Naturæ*, expressly says, that he never at any time observed them to get together any Food against Winter; and is of Opinion, that, during the Severity of the Winter, they eat nothing; as is common with many Insects, and some Species of Bees. His own Words, in *Vol. I. p. 296.* are as follow: *Neque etiam unquam observavi, quod cibaria quædam in hyemem sibi comparent: unde censeo ipsas, quousque vehementissima est hyems, nihil comedere; quemadmodum multis insectis, et apum quoque nonnullis speciebus, familiare est; quæ tempore brumali ab omni penitus cibo abstinent.*

Our Author, with great Deference to the Writers who have held the Affirmative, and with extreme Decency, differs from them, offering a handsome
Apology

Apology for himself. --- He suggests, that in warmer Regions they may not undergo the Chill they do with us ; and therefore may not pass the Winter in a State of Numbness. --- That, if this be the Case, a Store of Food must be necessary to them, which is not to our Northern Ants, which live, as it were, entranced. He adds, that, upon the most impartial Examination of Authors, the Opinion seems rather to be supported by its Antiquity, than reduced to a clear Demonstration. --- He tells us, that, as upon the most exact and frequent Examination of numerous Settlements, in the Winter, he could never trace out any Reservoirs of Corn, or other Aliment ; no, not in those of the Hill-Ants, which are the largest, and proportionably strong : So, to put this Matter beyond all reasonable Doubt, he had recourse to Experiments ; which, had the Supposition been true, could not probably fail of succeeding. At the Beginning of the Spring, he placed, in several Flower pots, and other Conveniencies, different Colonies of yellow small black Ants, &c. with their respective Queens, Attendants, and Vermicles ; in which Position they continued Summer, Autumn, and Winter, and carried on their Operations as in other Settlements, nourished their Young, and brought them to Perfection : From whence he concludes, that they would have laid up Provisions, had it been their Custom ; but, upon carefully examining some of these Pots, he found no Appearance of Magazines of Corn, or any collected Food : And that, upon his having frequently observed their Excursions from, and Return to their Colonies, he could never find, that they ever return'd with any Wheat, Corn, or any other

vegetable Seed; tho' they would with Eagerness attack a Pot of Honey, or a Jar of Sweetmeats, &c. Many other Experiments, besides these, our Author made, which I forbear to give you, judging these to be satisfactory.

The most material Argument in Favour of Ant-Magazines, he thinks, is the Authority of the Sacred Writings. --- *Solomon*, he says, has twice mention'd these extraordinary Insects; and each time with an immediate Reference to their Sagacity in providing for the Necessities of Winter. — For removing this Difficulty, he has recourse to the former Solution. — The superior Warmth of the Climate he lived in, and, of consequence, the proportionable Clemency of the Seasons; whence he concludes the Ants of those Countries may vary from ours in this, as well as in other Respects: Or perhaps, adds he, it might have been a received Opinion, as was the Sun's Motion; from whence this great Prince might recommend it, as a worthy Example of Industry and Wisdom.

If I might have Leave humbly to offer my Opinion, our Author seems to have justified his Conduct in departing from the commonly received Opinion: And perhaps there is good Reason to think that it has been handed down from ancient Writers of Reputation, and too easily received, without carefully examining into the Truth of the Fact; which Persons might easily be led to do from a general Observation of the extraordinary Industry of these little laborious Animals in carrying Things into their Cells.

The most learned *Bochart*, in his *Hierozyicon*, has display'd his vast Reading on this Subject, as he usually

usually does on all others; and has cited Passages from *Pliny*, *Lucian*, *Ælian*, *Zoroaster*, *Origen*, *Basil*, and *Epiphanius*, *Jewish Rabbi's*, and *Arabians*, all concurring in the Opinion, that Ants cut off the Heads of Grain, to prevent their germinating: But he confesses, that the ancients *Greek* Writers have made no such Observation of the Ants; nor any of them who lived before *Pliny*, as far as he remembers. Very probably this Opinion arose from what might have been observed of these laborious Insects, in cutting asunder with their Saws such Grains of Corn, or other Matters, which they might have Occasion to carry to their Nests, but were too bulky; for that they cut off Grass, and other Things, which they find in the Road to and from their Repositories, our Author has observed: And it is observable, that the *Hebrew* Name of the Ant נמלה *Nemala*, from the Verb נמל *Namal*, which signifies to cut off, is used for cutting off Ears of Corn (*Job*, xxiv. ver. 24. *)

But if we consider the two Texts, in the Book of *Proverbs*, cited by our Author, there is not the least Intimation in them of their laying up Corn in Store *against Winter*. In *chap. vi. ver. 8.* it is said, *She provideth her meat in the summer, and gathereth her food in the harvest*: For, tho' the former Verb הכין *Hekin* signifies to prepare, or dispose in Order, and the latter אגר *Agar* to collect, or gather together;

* I might also have referred to the *Theatrum universale omnium Animalium* of *Jonston*, publish'd by Dr. *Ruyfch* junior of *Amsterdam*, in 2 Volumes folio, Vol. II. p. 85.

together; and in the only two Places where I find it occur besides, is used for Gathering in Summer, as *Prov.* x. 5. and for Gathering in the Vintage, *Deut.* xxviii. 39. yet the Expressions, in the Text, necessarily mean no more, than that they collect their Food in its proper Season; — nor is there any thing else declared *chap.* xxx. *ver.* 25. So that all which may fairly be concluded from Scripture is, that they carry Food for themselves into their Repositories *. That they do this against *Winter* can only be determined by examining into the Fact: This our Author has done with very great Diligence, and has discovered, with respect to our *English* Ants, that they eat not at all in the Winter and have no Stores laid in of any sort of Food. The Opinion therefore of their laying in Magazines against Winter, seems to me to have been grafted on these Scriptures, rather than found in them; and this from a Conclusion naturally enough made, from observing (as I said) their wonderful Labour and Industry in gathering their Food in the Summer, — supposing that this must be to provide against Winter. — And, after all, great Part of their Labour, which may have been bestowed in other Services, might easily be mistaken, by less accurate Observers, for carrying in Food.

I am sorry I must omit the ingenious Author's just moral Reflections; but my Time will only allow me to conclude, as he does, with the Words of the Royal Psalmist, *Great is the Lord, and marvellous, worthy*

* *i. e.* To serve them as long as it will keep good, or they shall need it.

worthy to be praised, and there is no End of his Greatness. I am,

Dear Sir,

Tooting, Dec. 10.
1746.

Your most affectionate,

and obliged humble Servant,

H. Miles.

V. *A Remark on Father Hardouin's Amendment of a Passage in Pliny's Natural History, Lib. II. § LXXIV. Edit. Paris. folio, 1723. by Martin Folkes, Esquire, Pr. R. S.*

Read Jan. 22.
1746-7.

*V*asque horoscopa non ubique eadem sunt usui, in trecentis stadiis, aut ut longissime, in quingentis, mutantibus semet umbris solis. Itaque umbilici (quem gnomonem appellant) umbra in Ægypto meridiano tempore, æquinoctii die, paulo plusquam dimidiam gnomonis mensuram efficit. In urbe Roma nona pars gnomonis deest umbræ. In oppido Ancone superest quinta. Decima in parte Italiæ, quæ Venetia appellatur, eisdem horis umbra gnomoni par fit.

The geographical Reader cannot but observe here immediately, that somewhat is faulty in this Passage as it stands; since the equinoctial Shadow of the *Gnomon* being made shorter at *Ancona* than at *Rome*, the Latitude of *Ancona* will consequently be made lesser than that of *Rome*; whereas it is known to
be

be considerably greater; *Ancona* standing on the *Adriatic*, about two Degrees to the North of that Capital.

But, upon turning to Father *Hardouin's* Observations upon this Passage, I find the Text to have been altered by him in a very remarkable Manner, from all the former printed Editions. His Observation is as follows :

Hactenus editum est, in oppido Ancone superest quinta XXX. In parte Italiae, &c. MSS. hic variant. Nos ex certissima conjectura edi curavimus, Ancone superest quinta. decima in parte Italiae, &c. In decima regione Italiae Venetiam statuit in geographicis libro sequente. Neque simile veri est, in tot particulas, hoc est, in quintas tricesimas, ab homine πλεσιχώτερος ubique agente, gnomonem umbramve dividi.

Upon which Words it may be noted, that altho' the Reverend Father acquaints us he had met with some Variation in the Manuscripts, yet he appeals to none; nay, he even tells us expressly, that his Amendment was purely made upon Conjecture; whence we may safely conclude, that it stands unsupported by any various Reading or Authority whatsoever. He at the same time also acknowledges, that all the printed Editions conspire in another Reading; which I have found to be true in several I have had Occasion to look into, with this only Variation, that whereas the first Edition in 1469, and several of the following ones, print the Word *quinta* at Length, and XXX only in Figures; some of the later ones, and that by the *Elzevirs* particularly in 1635, print both the Words at Length; the whole Passage running thus :

Umbilici (quem gnomonem appellant) umbra in Aegypto meridiano tempore, æquinoctii die, paulo plus

plus quam dimidiam gnomonis mensuram efficit: in urbe Roma nona pars gnomonis deest umbræ: in oppido Ancone superest quinta trigesima: in parte Italiae quæ Venetia appellatur, iisdem horis umbra gnomoni par fit.

The plain Meaning of these Words is only this, that the Length of the Shadow of a *Gnomon* or upright Style at Noon, on the Day of Equinox, is, in *Egypt*, little more than half the Height of the *Gnomon*; that the same at *Rome* wants a ninth Part of that Height; that at *Ancona* the Height of the *Gnomon* exceeds the Length of its Shadow, by one thirty-fifth Part, or is in Proportion to it as 35 is to 34; and that, in the Part of *Italy* which is called *Venetia*, the Length of the Shadow, and the Height of the *Gnomon*, are equal to each other.

The Particulars here mention'd are respectively true, in the 4 following Latitudes, $26^{\circ} 34'$, $41^{\circ} 38'$, $44^{\circ} 10'$, and 45° . The first of which is the Latitude of the middle Parts of *Egypt*, and the last that of several Places in the Territories of *Venice*, the City itself standing, according to *Manfredi's* Table, in the Latitude of $45^{\circ} 33'$, and *Padua* in that of $45^{\circ} 28'$. The Latitude of this last Place is given by *Ptolomy* $44^{\circ} 36'$, and that of *Aquileia* at the Head of the *Adriatic* in the *Friuli* exactly 45 Degrees.

The Latitude of *Rome*, according to *Ptolomy* $41^{\circ} 40'$, only exceeds that collected from the Text before us by 2 Minutes; and his last again falls short $15\frac{1}{2}$ of that delivered by *Manfredi* and *Bianchini* $41^{\circ} 54\frac{1}{2}$. Besides which it may be noted, that the very Fact here mention'd is also spoken of by *Vitruvius*, as Father *Hardouin* has himself in another of

his Notes observed. *Sol æquinoctiali tempore arête libræ versando, quas ex gnomone partes habet novem, eas umbra facit octo, in declinatione cæli, quæ est Romæ.*

We come then to the Latitude of *Ancona*, which is given by *Manfredi* $43^{\circ} 54'$, or $16'$ less than that above collected from *Pliny*; but which is set down by *Ptolemy* $43^{\circ} 40'$, half a Degree less than the same. This may however be looked upon as no bad Observation, considering the Time when, and the Manner how, it is supposed to have been made, as we are ignorant of the Hour when the Sun really crossed the *Equator* on the Days of Observation; and especially as it comes, with all its Imperfections, as near to the Truth, as that reported both by our Author and *Vitruvius* to have been made at *Rome* itself; and as it only exceeds the true Latitude by about the same Quantity, which that given by *Ptolemy* seventy or eighty Years afterwards fell short of the same.

It therefore appears, upon the whole, that this Text needed no Correction; and for the Observation, that 35 Parts were too many for a *Gnomon* to be divided into, it will be found to have very little Weight, when it is considered, that the Antients made use of very large *Gnomons* upon these Occasions; that one of the Obelisks now standing at *Rome*, that of *St. John's Lateran*, is in Height 108 *English* Feet without the Pedestal; and that the other, still buried under the *Campo Marzo*, which was formerly used for this very Purpose, wanted but little of the same Height. The thirty-fifth Part therefore of the Height of such a Stone, did not fall short

short of three *English* Feet; a much less Quantity than which would easily discover itself in the Shadow, whose Length, notwithstanding all Difficulties arising from the *Penumbra*, might certainly be determined to less than half a Foot.

I shall just add to this Remark the Description given by *Pliny* of this *Gnomon*; who, speaking in his xxxvi. *Book*, § XIV. of the Obelisks that were at *Rome* in his Time, adds, in the Beginning of § XV. *Ei, qui est in campo, divus Augustus addidit mirabilem usum, adprehendendas solis umbras, dierumque ac noctium ita magnitudines, strato lapide ad magnitudinem obelisci, cui par fieret umbra, brumæ confectæ die, sexta hora, paulatimque per regulas (quæ sunt ex ære inclusæ) singulis diebus decresceret, ac rursus auferet.* From which Description I understand, that there was laid down, from the Foot of the Obelisc Northward, a level Pavement of Stone, equal in Breadth to the Breadth of the Obelisc itself, and equal in Length to its Shadow at Noon upon the shortest Day; that is to say, that its Length was to the Height of the Obelisc almost as 22 are to 10; and that into this Pavement there were properly let in parallel Rulers of Brass, whose Distances from the Point, directly under the *Apex* of the Obelisc, were respectively equal to the Lengths of the Shadow thereof at Noon, on the several Days of the Year; as the same Lengths decreased from the shortest Day to the longest, and again increased from the longest Day to the shortest.

After which the Author mentions in a Passage greatly corrupted, and therefore now almost unintelligible; that one *Manilius*, or *Manlius*, had added

to the Top of the Obelisc a gilded Ball, whose Use was to make the Shadow of the Extremity the more observable, as the middle Part of the Shadow of that Globe could readily be estimated; whereas the Shadow of an *Apex* would, at so great a Distance, be intirely imperceptible.

VI. *A Letter from the Rev. Mr. Mafon, Woodwardian Professor at Cambridge, and F. R. S. to the Pr. R. S. concerning Spelter, Melting Iron with Pit-coal, and a burning Well at Brofley.*

S I R,

Read Jan. 22. 1746 7. **H**AVING met with several Things, in a Ramble last Summer, that were new to me, and imagining they might be so to you likewise, and being of some Consequence, I presume to trouble you with a short Account of some of them.

What *Spelter* is I don't well know, nor what Uses are already made of it; but I believe it was never yet applied to so large a Work as the Cylinder of a Fire-Engine, till Mr. *Ford*, of *Colebrook Dale* in *Shropshire*, did it with Success: It run easier, and cast as true as Brass, and bored full as well, or better, when it had been warmed a little: While cold, it is as brittle as Glass, but the Warmth of my Hand soon made it so pliant, that I could wrap a Shaving of it round my Finger like a Bit of Paper. This Metal

tal never rusts, and therefore works better than Iron; the Rust of which, upon the least Intermiſſion of working, reſiſts the Motion of the *Piſton*.

Several Attempts have been made to run *Iron Ore* with Pit-coal; I imagine it hath not ſucceeded anywhere, becauſe we have had no Account of its being practiſed; but I find that Mr. *Ford*, from Iron Ore and Coal, both got in the ſame Dale, makes Iron brittle or tough, as he pleaſes; there being Cannon thus caſt ſo ſoft as to bear Turning like wrought Iron.

At *Broſeley*, about a Mile from the fore-mention'd Place, in the Year 1711, was a Well found, which burned with great Violence, whereof ſome Account is given in *Philof. Tranſact.* N^o. 334; but it has been many Years loſt. The poor Man, in whoſe Land it was, miſſing the Profit he uſed to have by ſhewing it, applied his utmoſt Endeavours to recover it; but all in vain, till *May* laſt; when, attending to a rumbling Noiſe under the Ground, like what the former Well made, tho' in a lower Situation, and about 30 Yards nearer to the River, he happen'd to hit upon it again.

That you may have ſome Notion of what it is, I will lay before you ſuch an Account of it, as the curſory View I had will permit.

The Well for 4 or 5 Feet deep is 6 or 7 Feet wide; within that is another leſs Hole, of like Depth, dug in the Clay; in the Bottom whereof is placed a cylindric earthen Veſſel, of about 4 or 5 Inches Diameter at the Mouth, having the Bottom taken off, and the Sides well-fix'd in the Clay ramm'd cloſe about it.

it. Within the Pot is a brown Water, thick as Puddle, continually forced up with a violent Motion, beyond that of boiling Water, and a rumbling hollow Noise, rising and falling by Fits 5 or 6 Inches; but there was no Appearance of any Vapour rising; which perhaps might have been visible, had not the Sun shone so bright.

Upon putting down a Candle at the End of a Stick, at about a Quarter of a Yard Distance, it took Fire, darting and flashing in a violent Manner, for about half a Yard high, much in the manner of Spirits in a Lamp, but with a greater Agitation. The Man said, that a Tea kettle had been made to boil in 9 Minutes Time; and that he had left it burning 48 Hours together, without any sensible Diminution.

It was extinguished by putting a wet Mop upon it, which must be kept there a small time; otherwise it would not go out. Upon the Removal of the Mop, there succeeded a sulphureous Smoke, lasting about a Minute; and yet the Water was very cold to the Touch.

The Well lies about 30 Yards from the *Severn*; which, in that Place, and for some Miles above and below, runs in a Vale full 100 Yards perpendicular below the Level of the Country on either Side, which inclines down to the Country at an Angle of 20 or 30 Degrees from the Horizon; but somewhat more or less in different Places, according as the Place is more or less rocky.

The Country consists of Rock, Stone, Earth, and Clay, unequally mix'd; and as the River, which is very rapid, washes away the soft and loose Parts, the next successively slip into the Chancel; so as, by
degrees,

degrees, and in time, to affect the whole Slope of the Land : And as the inferior *Strata* yield Coal and Iron-Ore, their Fermentation may produce this Vapour, and force it to ascend with Violence through the Chinks of the Earth, and give the Water the great Motion it has. This might be obstructed in one Place by the foremention'd subsiding of the sloping Bank, and might afterwards find a Vent in another ; in like manner as it happen'd at *Scarborough Spaw*, a few Years since.

If these Hints should be any Amusement to you, or be the Means of setting any more able Person upon further Inquiries, and giving a better Account of them, I have all that is intended by

Jan. 18. 1746.

Your humble Servant,

Chas. Mason.

VII. *Part of a Letter from Mr. John Brown-
ing, of Bristol, to Mr. Henry Baker,
F.R.S. dated Dec. 11. 1746. concerning
the Effect of Electricity on Vegetables.*

Read Jan. 22.
1746-7.

HAVING an Operator at *Bristol* with a good electrifying Machine, I was desirous to electrise a Tree, and therefore sent him the following for that Purpose; viz. *Laurustinus*, *Leucoium majus flore pleno ferrugineo*, and *Stæchas citrina Cretica*. These were not chosen with any

Design; their being the least Plants I had, was the only Reason.

I promised myself the Pleasure of seeing their Leaves erected when electrified, but was disappointed (whether its being the dormant Season of the Year for all Plants might not be some Hindrance, I cannot determine); neither did the Leaves flag on their being touched. However, I was agreeably recompensed, by a Stream of fine purple blue coloured Light, much resembling an Amethyst, that issued from the Extremity of each Leaf upwards, of an Inch in Length, when the Finger, or any other Non-electric, approached near it. This Colour I attribute to the watry Particles in the Earth, having often observed the very same Colour issuing from the long Leg of a Syphon. On putting my Finger on the Gun-barrel to stop the Electricity, the Leaves of each Tree had a trembling Motion, which remained for some little Time, and immediately ceased on withdrawing my Finger from the Barrel, and admitting the Electricity. This constantly happened, as I put my Finger on or off the Barrel.

The *Stæchas* Plant has a very long hoary Leaf, and bears its Blossom on a very small, slender, and almost naked Stem, rising near a Foot above the Body of the Plant. This Stem had a Motion given it, when any Non-electric was brought within about two Inches of its Summit, much like the Vibration of the *Pendulum* of a Clock; which vibrating Motion was parallel with the Breech of the Gun, quite contrary to the same kind of Motion I had before observed in a Needle, hanging perpendicularly by a Thread at the End of the Gun; the Needle always
vibrating

vibrating in the Direction of the Gun. The Motion of the Plant and Needle always continued as long as the glass Globe was excited.

I was also desirous to be satisfied, whether Electricity could be propagated without mutual Contact, by suspending another Gun in Silk Cords, about two Inches from Contact, and the Electricity was near as strong in the second Gun as in the first. At the Distance of between 3 and 4 Inches it was much abated, and so it gradually diminished, as the Distance increased to near 6 Inches, where it would scarce attract a Thread of Trial.

I prevailed on a Man to be let Blood, and then placed him on a Cake of Pitch, but could not be sensible of any Increase of Velocity in his Blood, by being electrized, as has been asserted.

I had almost forgot to mention, that the Strokes I received from the electrified Garden-Pots were more violent and painful to my Fingers than from any other Body I ever experienced.

Mr. Baker, since his receiving the above Account, has had an Opportunity of electrifying a Myrtle-tree, of between 2 or 3 Feet in Height, growing in a Pot, at the Seat of the Duke of *Montague* at *Ditton*; in Presence of His Grace, of the *President* of this *Royal Society*, and several other curious Gentlemen; who found, that whenever the Hand, or other non-electric Body, was brought near the Leaves, Streams of fine purple Fire issued therefrom, together with a considerably cold Air; and that the Leaves would be attracted at some Distance, and move vigorously towards a non-electric Body.

VIII. *Mercurius* sub Sole visus in Specula
 astronomica Academiae *Giesensis*, Anno
 1743. die 5 *Nov. a Christiano Ludovico*
Gersten, Math. Prof. et R. S. *Lond.* Sod.

Read Jan. 22.
 1746-7.

IN observatione hujus transitus tribus
 potissimum machinis usus sum, quarum
 brevem descriptionem ut præmittam, instituti ratio
 ante omnia postulabit.

Prima atque præcipua erat tubus astronomicus 10
 pedum bonæ notæ, cui aptavi micrometrum exqui-
 siti operis *Londini* comparatum. Præcipua structuræ
 in eo consistunt. In centro tubi loco convenienti ad
 angulos rectos sese decussant duo fila immobilia, et
 alia rursus duo, situ ad unum mobilem parallelo per
 motum unius cylindri chalybei duplici cochlea (qua-
 rum una ad dextram, altera ad lævam vergit) æqualibus
 helicibus striati, continue et æqualiter pro diverso
 cochleæ motu aut versus centrum progrediuntur, aut
 ipsum post se relinquunt, addita machinula peculiari
 duobus indicibus instructa, quorum unus numerum
 revolutionum integrarum cochleæ, alter numerum
 divisionum unius revolutionis monstrat. Hoc omne
 rursus per rotam dentatam et cochleam aliam perpe-
 tuam, ubi opus est lento, et exacto motu circumagi,
 atque ad quosvis angulos et circulos cœli aptari po-
 test. Hoc micrometrum ea *Cassiniana* methodo, quæ
 usitatiores est ad transitum observandum, applicavi.
 Radebat nempe *Mercurius* filum immobile ad æqua-
 torem parallelum, dum ex mobilibus unum solis mar-
 ginem inferiorem attingebat. Machinæ parallacticæ ap-
 paratu

paratu facile carui; nam adducto semel Mercurio ad centrum tubi, rotæ dentatæ atque cochleæ perpetuæ artificio, brevi mora filum immobile in eum situm redigebatur, ut Mercurius super istud incederet.

Secundum instrumentum erat horologium oscillatorium astronomicum, *Londini* ab egregio hoc in genere artifice *Joh. Ellicott*, R. S. Sod. confectum; de cujus exacto satiss, et ad medium motum solis accommodato motu, complurium annorum observationes nullum mihi dubitandi locum relinquunt. Ope hujus automati appulsuum momenta tam Mercurii quam limborum solis ad filum horarium adnotavi.

Tertia machina ad tempus verum meridiei invenientium inserviebat. Ob defectum quadrantis astronomici eam ad usus meos inveni atque effeci, prolixiorē descriptionem alia fortassis occasione daturus. Quæ vero præcipua sunt, huc redeunt: ut ejus ope momenta temporis complurium duarum æqualium altitudinum solis ante et post meridiem ad 2, 4, summum 6 minutorum secundorum certitudinem (pro ratione nimirum obliquitatis arcus, quem sol transcurrit motu diurno ad lineam horizontalem) determinare atque exinde meridiei verum tempus ad 1, 2, summum 4 sec. certitudinem indagare possum. Et hac quidem ipsa quinta die *Nov.* per duas bonas cum hoc instrumento confectas observationes, quibus sequenti sexto hujus mensis die, alias tres addere contigit, horologii tempus ita correxi, ut citra 4 vel maxime 5 minutorum secundorum errorem, recte istud factum fuisse existimem. Præmissis hisce, ad phænomena me convertō.

Paulo post horam matutinam 8, nubes quibus totum cœlum foede obductum erat, præter expectationem

dehiscere cœperunt, atque brevi intervallo per hiatum satis magnum sol apparebat purissimo lumine fulgens. Tubo statim applicato cum nihil adhuc de Mercurio, nullaque prorsus macula in eo conspiciebatur, id egi, ut diametrum ejus horizontalem repetitis compluribus observationibus capefferem, quod etsi ob motus rapiditatem non ita facile fuerit factu, existimavi tamen non male tandem deprehensam fuisse semidiametrum ejus micrometri rev. 21. et $\frac{22}{7}$. Verticalem semidiametrum postea ad hor. 11. min. 20. circiter reperi exacte 21. rev. $\frac{18}{7}$. Quantum hi numeri in partibus circuli maximi efficiant, istud infra indicabitur.

Capta semidiametro solis horizontali, densissimæ rursus nubes eum occultabant; sed hor. 9. min. 6. sec. 25. subito Mercurium in ejus disco conspexi, jam totum, si recte memini, ingressum in margine tamen adhuc hærentem. Forte nomenclator, qui horologii oscillatorii numeros viva voce mihi indicaret, aberat: quapropter statim ab hoc spectaculo ad horologium temporis, notitiam ut caperem, evolavi, in reditu reliqua observaturus. Nam ultra 5 passus non distabat, sed eo situ, ut a tubo ad indices non pateret prospectus; ast scriptis in schedam numeris rectum nubibus solem reversus inveni: inde factum ut non audeam affirmare, quod exacte tunc temporis in contactu marginis solaris constitutus fuerit.

Quæ nunc sequuntur observationes, nubium intervalla concessere. Favebat tamen aëris tranquillitas, favebat et absentia multorum spectatorum. Corpusculum Mercurii rotunde nigrum margine determinata absque ullis atmospheræ vestigiis apparebat, sed tantæ exilitatis, ut quantum nudo oculorum judicio

dicio conjectare licuit, non multum ultra duplum crassitiei fili immobilis in micrometro assurgeret, quod ex pilo capitis humani constabat. Circa hor. 1. post mer. min. 10. usque ad egressum sat magno intervallo nubes patebant; sed purior aër atque longior solis mora in eo, circa parietem observatorii, indirectum tunc ferme soli obversum calorem effecit: inde trepidatio atque undulatio limbi admodum importuna quam nullis remediis amovere potui.

Prima sequentis tabulæ columna exhibet tempus horologii. Secunda tempus verum correctum. Tertia, intervalla temporum, ab appulsu limborum solis ad appulsus Mercurii ad filum horarium, reducta atque conversa in minuta secunda circuli maximi, pro declinatione solis 15 gr. 39 min. 18 sec. Quarta, observationes. Quinta, distantias Mercurii a limbo inferiori solis, in partibus micrometri. Sexta denique, micrometri partes ad scrupula secunda circuli maximi reductas. Basis reductionis est: 23 revolutiones integre dant $17' 33'' \frac{1}{2}$. Quam ex solis et fixarum nonnullarum transitu talem inveni.

Temp. hor. ante merid.	Temp. verum correctum.	Dist. Merc. in A.R.	Observatione.	Rev. micr.	Dist. in Decl.
09 6 25	9 5 55	00	1. ☽ tot. ingress. vidi aut certe quoad maximam partem.		
9 44 7 0 0 55	9 43 37 0 44 25	694	2. ☽ ad horarium limb. solis se- quens ad horar.		
9 49 5 0 0 55	9 48 35 0 49 25	722	3. ☽ ad horarium limb. solis seq. ad horar.		
9 56 30 0 57 23	9 56 0 0 0 53	766	4. ☽ ad horarium limb. solis seq. ad horarium per nube- culam tenuem obs.		
10 2 43 0 3 38	10 2 13 0 3 8	794	5. ☽ ad horarium limb. solis seq. ad horar.		
10 44 35 0 45 44	10 44 5 0 45 14	996	6. ☽ ad horar. limb. solis seq. ad horar.		
10 48 14 0 49 25	10 47 44 0 48 55	1028	7. ☽ ad horar. limb. solis seq. ad horar.		
10 53 26 0 54 39	10 52 55 0 54 9	1054	8. ☽ ad horar. limb. solis seq. ad horar.		
11 33 52 0 35 20	11 33 22 0 34 50	1272	9. ☽ ad horar. limb. solis seq ad horar.		
11 40 54 0 42 25	11 40 24 0 41 55	1310	10. ☽ ad horar. limb. solis seq. ad horar.		
11 43 52 0 44 35 0 46 7	11 43 22 0 44 5 0 45 37	621 1329	11. Limb. solis preced. ad hor. ☽ ad horarium. Limb. solis seq. ad horar.		

Tempus ho- rologii.	Temp. verum correctum,	Diff. Merc. in A. R.	Observationes.	Rev. micr.	t. cl.
A. M. h ' "	P. M. h ' "				
11 57 42 0 58 20 0 59 57 P. M.	11 57 12 0 57 50 0 59 27	549 1401	12. Limb. folis præced. ad hor. ☿ ad horarium. Limb. folis seq. ad hor.	12 ⁴ / ₂	580
12 3 27 0 4 3 0 5 42	12 2 57 0 3 33 0 5 12	520 1430	13. Limb. folis præced. ad horar ☿ ad horar. Limb. folis seq. ad horar.	13 ² / ₂	600
12 46 56 0 47 16 0 49 11	12 46 26 0 46 46 0 48 41	289 1661	14. Limb. folis præced. ad horar ☿ ad horar. Limb. folis seq. ad horar.	15 ⁴ / ₂	715
1 16 56 0 17 8	1 16 28 0 0 38	144	15. Limb. folis præced. ad horar ☿ ad horar. Limb. folis seq. ad horar.	17 ² / ₂	788
1 19 27 0 0 35 0 21 41	1 18 57 0 19 5 0 21 11		16. Limb. folis præced. ad horar ☿ ad horar. Limb. folis seq. ad horar.	17 ² / ₂	793
1 36 15 0 0 20 0 38 19	1 35 45 0 35 50 0 37 49		17. Marginem int. tangere vide- atur, certe tetigit aut paulisp uperavit, penitus in limbo manuit ingens undulario ac repitatio limbi		
1 19 26 ¹ / ₂ 0 0 35 0 21 41 ¹ / ₂	1 18 56 ¹ / ₂ 0 19 5 0 21 11 ¹ / ₂	122 1828	Obs. 16. Ob trepidationem limbi fo- liarias ad analog. reliquorum sic corrigendam duco. Limb. folis præc. ad horar. ☿ ad horar. Limb. folis seq. ad hor.	17 ² / ₂	793

Nunc ad corollaria progredior ex his observationibus derivanda. Ante omnia diameter solis determinanda: ut id rite fiat, opus est ut habeamus declinationem, necnon altitudinem ejus eo tempore quo diameter verticalis mensurata fuit. Declinatio solis facile ex longitudine ejus supputatur. Longitudinem ex tabulis *Ludovicianis* ad hor. 11. 20' 39'' temporis veri (medium scilicet propemodum transitus) pro meridiano 25'. 10''. temporis a *Parisiensi* versus orientem distantis deprehendi m 12°. 37'. 37''. Huic longitudini respondet declinatio australis 15°. 39'. 18''. Intervallum temporis ab appulsu limbi solis præcedentis ad appulsu sequentis per obs. 11, 12, 13, et 14, est 2'. 15''. Quod tempus in arcus æquatoris conversum dat 33'. 45''. Quodsi igitur hic arcus pro declinatione inventa secundum sphericæ doctrinæ regulas ad partes circuli maximi reducatur, prodit solis diameter 32'. 30''.

Porro ex observationibus astronomicis *Philippi Butisbacensis* Hassiæ *Landgravii* latitudo urbis *Butisbaci* ultra 4 horas itineris a *Giesæ* non distantis est 50°. 28'. Quare pro latitudine *Giesensi* accipio 50°. 30'. inde altitudo solis tunc temporis quo diameter ejus verticalis mensurata fuit, est præter-propter inter 23 et 24 gradum. Semidiameter solis in partibus micrometri erat 21 rev. et $\frac{7}{8}$ quod secundum tabulam meam = 16'. 13''. circuli maximi. Inde diameter verticalis eo tempore 32'. 26''. Sed propter refractionem ea justo minor appareri debuit, proditque ex tab. v. *Hireana* defectus iste 4''. addito hoc habemus rursus 32'. 30''. Sin adhibeamus tabulam refractionum recentiore ex hypothesibus *Tayllorianis* constructam, quam publicavit celeberrimus

Halleius,

Halleius *, atque reliquis præfert; defectus iste nonnulla scrupula tertia tantummodo prodibit minor.

Horizontalem semidiametrum ut supra dixi deprehendi in partibus micrometri 21 rev. $\frac{3}{2}$. Duplum hujus quantitatis † dat secundum tabulam meam 32'. 31''. circ. max. Ergo hæ tres observationes satis bene inter se consentiunt, atque diametrum solis constituunt 32'. 30''.

Pergo nunc ad angulum visum semitæ apparentis Mercurii cum ecliptica. Extra controversiæ aleam hoc positum existimo, quod Angelus sit oporteat, isque divinis machinis instructus, qui complures ejusmodi observationes ita perficeret, ut omnia Mercurii loca per easdem determinata exacte in unam eandemque lineam coincidant. Nam ut taceam refractiones atque parallaxes, quas methodus adhibita non excludit, error semiscrupuli secundi temporis, 7 propemodum minuta secunda in distantia Mercurii a limbo solis sequenti vel præcedenti efficit. Modum igitur quem ob eandem rationem celeberr. *Manfredi* in transitu anni 1736 §, necnon alii se adhibuisse testantur, elegi. Exacta nimirum scala summaque cura omnes in typum retuli; quo pacto deprehendi: quodsi inter loca obs. 15 et 16 correctæ arithmeticæ medius locus quærat, deinde per hunc, necnon per cum quem
obs.

* *Phil. Transf.* No. 368. Vide *The Abridgment* by Mr. Reid and Gray, Vol. VI. p. 160.

† Nam per revolutionem unius cylindri duo fila mobilia simul aut centrum appropinquabant, aut ab eo discedebant, sicuti supra in descriptione micrometri indicatur. Ergo numeri machinæ non diametrum, sed semidiametrum indicabant.

§ *Phil. Transf.* No. 446, p. 106.

obl. 5 determinat, recta ducatur eam quam proxime veram semitam apparentem in disco solis exhibere. Hoc principio posito numeros adduxi. Distantia media inter obl. 15 et 16 correct. a limbo solis sequenti est 1817'' circ. max. Distantia media Mercurii inter easdem observationes a limbo inferiori solis 790'' $\frac{1}{2}$. Distantia loco obl. 5 a limbo sequenti est 794''. Distantia a limbo inferiori 288''. Ergo differentia inter distantias a limbo seq. 1023''. Differentia inter distantias a limbo inferiori = 502'' $\frac{1}{2}$. Hæ differentiæ ergo triangulum rectangulum constituunt, quarum prior pro basi altera pro catheto habenda. Ducto igitur calculo angulus ad basin provenit 26°. 9'. cui angulus semitæ cum circulo ad æquatore parallelo æqualis. Eodem modo angulos singularum locorum ab obl. 7. incipiendo cum loco observationis 5. quæfivi, et profiluere sequentes.

Ex obl. 5 et 7	angulus est	26 33
5 et 8	—	26 33
5 et 9	—	26 5
5 et 10	—	26 51
5 et 11	—	26 21
5 et 12	—	25 41
5 et 13	—	26 7
5 et 14	—	26 13
5 et 15	—	26 17

Medium 26 11

Quare cum priori casu angulus ad basin sit 26°. 9'. hoc vero 26°. 11'. medium sc. 26°. 10'. pro angulo

gulo viso semitæ cum circulo parallelo accipio. Erit ergo ang. semitæ apparentis cum horario $116^{\circ}. 10'$. Sed ad locum solis $12^{\circ}. 37'$. μ respondet per. tab. *Hircanæ* ang. eclipticæ cum meridiano $107^{\circ}. 43'$. Ergo ang. semitæ apparentis Mercurii cum ecliptica $8^{\circ}. 26'$.

Pro distantia centrorum minima elegi duas observationes, inter quas mediam viam tenere semitam typus indicabar, nec tamen multum ab ipsa semita distabant, septimam nempe et decimam. A distantis Mercurii a limbo inferiori subdixi $8''$, tribuendo $5\frac{1}{2}''$ pro semidiametro Mercurii*, reliquum vero dimidio crassitie fili paralleli. Nam ex constructione micrometri, in introductione exposita, claret, distantias a centro tubi, non vero a margine fili, assumendas esse: deinde ex distantia Mercurii a limbo sequente in obs. 7. semidiametro solis, angulo invento semitæ cum circulo parallelo, per triangulorum analysin, inveni semitæ sive centrorum solis et Mercurii distantiam minimam $9'. 2''$. ex observ. 10: vero simili calculo eadem distantia prodit $9'. 7''$. medium ergo, $9'. 4\frac{1}{2}''$. assumo pro vera distantia semitæ a centro solis. His præmissis trigonometrico calculo deduxi sequentia.

		1	11
Semitæ visæ longitudo in disco solis,	————	26	57
Latit. visæ Mercurii in conjunctione,	————	9	10
Latit. Mercurii in ingressu	————	10	57
			Latit.

* Diametrum Mercurii per micrometrum ad tubum *Hugenianum* 120 pedum applicatum, determinavit celeberr. *Bradley*, anno 1723. $45''$. Vide *Phil. Transf.* N^o. 386, p. 229.

Latit. Mercurii in egressu,	—	—	5 59
Different. inter latit. in egressu et ingressu			3 58
Portio semitæ inter δ et medium transitus,			1 20

Quæ restant, tempus nempe conjunctionis, positio nodi, atque inclinatio orbitæ, ex his quidem immediate non eruuntur; nam requiritur adhuc exacta determinatio moræ centri Mercurii in disco solis, quam ex meis observationibus tuto determinare nequeo. Comparatis tamen intervallis temporum cum distantis complurium locorum in semita, inveni satis consentientem motum horarium $5'. 56''$. adeoque omnem centri Mercurii in disco moram quamproxime accedere ad horas 4. min. 33. Cumque 1 vel 2 minutorum temporis, hac in re, error exiguum in nodo imò in orbitæ inclinatione efficiat differentiam, breviter ea subjungere libet, quænam ex hoc hypothetico calculo provenire.

Ponamus igitur, quoniam trepidatio limbi verosimili ratione, contactum Mercurii ad interiorem marginem, adeoque et egressum anticipavit.

Tempus verum egressus centri γ in disco solis <i>Giesæ</i> ,	—	—	—	1 37 0
Dimidiam moram in disco	—			2 16 30
Erit medium transitus		<i>Nov. 4.</i>		23 20 30
Ex motu horario, et portione semitæ inter δ et medium transitus erit tempus per istam portionem	—			0 13 28
Ergo temp. ver. conjunct. <i>Giesæ</i> , <i>Nov. 4.</i>				23 7 2
Ponamus porro differentiam meridianorum inter <i>Giesæ</i> et obser. <i>Parisiinum</i> , reiectis secundis	—	—		0. 25 0
				Erit

Erit tempus verum conjunctionis in ob-
servat. *Parisin.* ——— ———

h ' "
22 42 2

Æquatio temporis ex tab. *Ludov.* sub. ———

0 20 24

Erit temp. med. conjunct. in obs. *Parif.* ———

22 21 38

Ad hoc tempus locus solis ex tab. ———

0 ' "

Ludovic. ——— ——— m

12 37 0

Porro ex differentia inter latit. in in-
gressu, et latit. in egressu, necnon latit.
in δ atque mora centri in disco, sc. h. 4.
m. 33. resultat tempus quod φ absol-
vit a δ ad δ ——— ———

h ' "
10 31 25

Ex tabulis *Ludovicianis*, a quibus *Ca-
rolinæ* hoc in passu vix differunt, hoc
temporis spatio Mercurius heliocentrice
in ecliptica progreditur ———

0 ' "
2 39 13

Ergo locus nodi his hypothefibus φ ———

15 16 13

Quodsi mora centri Mercurii suppo-
natur 4h. 32'. erit tunc δ ——— φ

15 15 38

Si vero mora centri φ supponatur
4h. 34'. erit δ ——— ——— φ

15 16 47

Quodsi ponamus distantiam Mercurii
a terra esse ad distantiam φ a \odot ut 676
ad 313. sicuti eam celeberr. *Halleius*
definit (vid. *Phil. Transf.* N°. 386. sive
The Abridgment, by Mr. *Reid* and
Gray, Vol. VI. p. 241.) erit inclinatio
 φ in conjunctione ——— ———

0 19 47

Ex hoc arcu atque distantia φ in eclip-
tica a δ consequitur tandem inclinatio
orbitæ, et quidem in primo casu, ubi
mora centri supponitur 4h. 33' ———

0 7 5
Quodsi

Quodsi mora centri \varnothing in disco suppo-	h	1	17
nitur 4 h. 32'. erit inclinatio orbitæ	o	7	6

Si vero eadem mora esset 4 h. 34'.			
tunc inclinatio orbitæ	o	7	5

IX. *Observations upon so much of Monsieur le Monnier the younger's Memoir, lately presented to the Royal Society, as relates to the communicating the Electric Virtue to Non-electrics; by Wm. Watson, F. R. S*

Read Jan. 29. 1746-7. **T**HE World is much obliged to Mons.
le Monnier for the many Discoveries he has made of the Power of Electricity, though the Reason of my troubling you with this Paper at this time, is my differing with that Gentleman in the Conclusions which he deduces from several of the Experiments contain'd in his Memoir lately presented to the *Royal Academy of Sciences at Paris*, his own Extract of which was lately communicated to the *Royal Society*. *

One of the Questions proposed to be examined is,
“ In what manner the electric Virtue is to be communicated to such Bodies as yet have it not, and
“ which are not capable of acquiring it by bare
“ Friction only?” Monsieur le Monnier observes hereupon, “ That no other Manner is known, by
“ which the electric Virtue may be communicated,
“ besides the near Approach of a Body actually possess'd of the same: That the Rule laid down by
Monsieur

* See These Transactions, No. 481, p. 291.

“ Monsieur du Fay, That Bodies never receive
 “ Electricity by Communication, unless they are
 “ supported by Bodies electric in their own Nature,
 “ does not always take place; and that it is liable
 “ to great Exceptions:” For, first, in the *Leyden*
 “ Experiment, the Phial filled with Water is strongly
 “ electrified by Communication, even when carried
 “ in the Hand, which is not a Body electric by
 “ Nature.”

To this I answer, that Monsieur du Fay's Rule is confirmed by all the Experiments yet made public, and even by that of *Leyden* quoted by our Author, or what is usually called that of Professor *Muschenbroeck*. For, in this Experiment, is not the non-electric Water contained in and supported by the glass Phial, which is electric in its own Nature? Its being carried in the Hand is no more than its being placed on any other non-electric Body, and therefore is no Proof against the general Position. It is well known, that if the Phial is made non-electric by wetting its Outside, so as not to leave some Inches perfectly dry, between its Mouth and that Part which is wetted, the Water and Phial part with the Electricity as fast as they receive it, unless it is stopped by another Electric *per se*. But of this I treated at large, in a Paper I lately did myself the Honour to communicate.

Secondly, our Author mentions, “ That all Bodies, which are electrified by means of a Phial of Water fitted to a Wire, and which has already received a great deal of Virtue by Communication, all Bodies, he says, placed in any curve Line, connecting the exterior Wire and that Part
 “ of

“ of the Bottle, which is below the Surface of the
 “ Water, acquire Electricity without being placed
 “ upon Resin, Silk, Glass, or the like : That thus
 “ a violent Concussion may be given to 200 Men
 “ all at once ; who holding each other by the
 “ Hand so form the Curve just mention’d, when
 “ the first holds the Bottle, and the last touches the
 “ Wire with the End of his Finger ; and this equally,
 “ whether they are all mounted upon Cakes of Re-
 “ sin, or stand upon the Floor : That the Electricity
 “ has in this manner been carried through a Wire
 “ of the Length of 2000 Toises, or near $2\frac{1}{2}$ *English*
 “ Miles ; Part of which Wire dragged upon wet
 “ Grass, went over Hedges, Palisado’s, and over
 “ Land newly ploughed up.”

The Experiments in the second Argument do no-
 ways invalidate Monsieur *du Fay*’s Rule ; for the
 Success of them depends upon keeping whatever
 forms the curve Line mention’d by our Author,
 whether it consists of Men or Wire, in a non-
 electric State : And if whatever forms this curve
 Line acquires any Degree of Electricity more than
 its original Quantity, which it is well known may
 be done, by being placed upon originally Electrics,
 the Effect of the Shock is proportionably lessened.
 Thus if a Man, standing upon Electrics *per se*, ap-
 plies his Hand to the Phial of Water, suspended by
 a Wire to the electrified Gun-barrel as usual, this
 Person will acquire Electricity, which will be suf-
 ficiently perceptible in him, by his attracting light
 Substances held near his Body, or by his firing in-
 flammable ones, when properly presented to him ;
 if, I say, a Person thus electrified, by applying one
 of

of his Hands to the Phial, touches the electrified Gun-barrel with a Finger of his other, let the Phial be ever so strongly electrified, he feels but a slight Stroke, and this Stroke is greater or less, in proportion to the Difference of the Accumulation of Electricity in the Body of the Man, and that of the Water in the Phial. Thus we know from Experiment, that though a considerable Quantity of the Electricity, in impregnating the Phial of Water therewith, pervades the Glass, yet the Loss thereof this Way is not equal to what comes in by the Wire: Therefore we will, for the sake of a more easy Method of Explanation, suppose, that the Phial, when electrified in the most perfect manner, contains a Quantity of Electricity equal to 10; that the Man's Body, by standing upon Wax, and touching the Phial with one of his Hands during its Electrification, contains a Quantity equal to 7: Upon his touching the Gun barrel with a Finger of his other Hand, he will receive a small Stroke only equal to 3, the Difference of the Electricity of the Water and that of his Body: And if he touches the Gun-barrel again, without removing his Foot from the originally Electric, the Stroke will be scarcely perceptible, on account of his Body being nearly of the same Degree of Electricity with the Water in the Phial. So that here we see that the Violence of the Shock, to be felt by whatever forms the curve Line, depends upon its being, in the most perfect manner, free from any Degree of Electricity more than the original Quantity; which is contrary to the Opinion of our Author.

Thirdly, Monsieur *Monnier* tells us, " That the Water of the *Bason* of the *Thuileries*, whose Surface is about an Acre, has been electrified in the following manner:

" There, was stretched round half the Circumference of the *Bason* an iron Chain, which was entirely out of the Water; the two Extremities of this Chain answer'd to those of one of the Diameters of the Octagon: An Observer, placed at one of these Extremities, held the Chain with his left Hand, and dipped his right at the same time into the Water of the *Bason*; whilst another Observer, at the opposite Side of the *Bason*, held the other End of the Chain in his right Hand, and a Phial, well electrified in his left. He then caused the Wire of his Phial to touch an Iron Rod, fixed upright in a Piece of Cork that floated near the Edge of the *Bason*. At that Instant both Observers felt a violent Shock in both their Arms. The same Fact was again confirmed by Experiments made upon two *Basons* at the same time, that it might appear distinctly, that the electrical *Effluvia* did really pass along the Surfaces of the Water."

The Water of the *Bason* in this Experiment was no more electrified than the Wire which dragged along the Ground, &c. was in the former. When I was first informed, without being acquainted how, that an Acre of Water had been electrified, I was amazed, and told the Gentleman who acquainted me therewith, that if my Idea of Electricity was in the least true, such an Effect could not be produced, without electrifying the whole terraqueous Globe from

from a larger Mass of Matter. And indeed, when I heard Monsieur *le Monnier*'s Paper read, I easily saw the Deception: So that, instead of electrifying the whole Quantity of Water contain'd in the Bason, the Electricity pass'd only through so much of it as formed a Line between the iron Rod fastened in the floating Cork, and the Hand of that Observer which was dipped in the Water.

These Experiments still more and more establish the Account I lately laid before you of the Electricity's always describing the shortest Circuit between the electrified Water and the Gun-barrel; or (which is the same thing) the Wire of the electrified Phial. And this Operation respects neither Fluids or Solids, as such, but only as they are non-electric Matter. Thus this Circuit, in the preceding Experiment between the Phial and the Wire, consisted of the two Observers, the iron Chain, the Line of Water, and the iron Rod in the floating Cork.

Fourthly, Monsieur *le Monnier* mentions, " That it has been confirmed, by repeated Comparisons, that a Bar of Iron, placed in the above-mention'd Curve, does not at all acquire more Electricity when it is suspended in silken Lines, than when it is held in the bare Hand: Whence it appears to him, that, in this Case, the contiguous non-electric Bodies do neither partake of, nor absorb in any way, the Electricity which has been communicated."

The curve Line before-mention'd, let it consist of whatever Non-electrics it will, unless the Whole thereof be properly supported, the communicated Electricity cannot be accumulated: So that the sus-

pending one Part thereof in silk Lines cannot be supposed to produce any Effect.

This Gentleman further observes, " That the Phial of Water fitted to its Wire does not receive the least Degree of Electricity, if its Wire, suspended by a silk Line, is applied to the Globe in Motion, or if that Phial is placed upon a dry glass Stand." This *Monsieur le Monnier* takes to be directly contrary to *Monsieur du Fay's* Rule; especially as the Phial cannot be replete with Electricity, unless, while it is exciting, some non-electric Body touches the Phial below the Water.

That the Phial of Water receives no Degree of Electricity in this Case is not strictly true. It receives as much, as any other Mass of Matter of the same Bulk would, under the same Circumstances. For we find, that we cannot highly electrify the Water, unless the Electricity from the Globe be directed through the Water and Phial to the Non-electric in Contact; in which Passage a great Quantity thereof is accumulated; by its not permeating the Glass so fast as it is furnished by the Wire; and therefore we find, that when the Water will contain no more, the Surcharge runs off by the Wire: So that this Experiment, no more than those which precede, contradicts *Monsieur du Fay's* Opinion; the Thickness of the Glass permitting it, not wholly, but partially, to stop the Electricity. This Matter is explained further under Experiment the first.

I differ from this ingenious Author with Reluctance, inasmuch as I greatly honour him, not only for his Discoveries upon the Subject of Electricity, but also for the Pleasure and Improvement I received in

in my reading his learned and curious Observations in Natural History, made in the Southern Parts of *France*, where he accompanied Monsieur *Cassini de Thury* in measuring a Degree of the Meridian. These Observations are published with Monsieur *Cassini's* Book : But as the reverse of several of the Opinions deliver'd in his Memoir is experimentally found to be true, and as the Discovery of Truth, and carefully separating it from Deception, should be the only Aim of our Philosophizing, I take the Liberty of laying before you my Opinion thereon.

X. *Abstract of a Letter from Mr. William Arderon, F. R. S. to Mr. Henry Baker, F. R. S. concerning the perpendicular Ascent of Eels.*

Norwich, July 9. 1746.

S I R,

Read Jan. 29.
1746-7.

WHEN I read, some Years ago, what Dr. *Plot* in his History of *Staffordshire* relates concerning the Passage of Eels across Meadows, in the Night-time, from Pond to Pond, I could hardly forbear thinking, that the Gentleman there mention'd must by some means or other have been deceived; but what I have lately seen with my own Eyes gives me great Reason to believe his Account to be strictly true.

On the 12th Day of last *June*, whilst I was viewing the Flood-Gates belonging to the Water-works in this City of *Norwich*, I beheld a great Number
of

of Eels sliding up them and the Posts adjacent, notwithstanding they all stood perpendicular to the Horizon, and 5 or 6 Feet above the Surface of the Pool below the Water-works. They ascended these Posts and Gates, until they came into the Dam above : And what makes the Matter appear still more strange, they slid up with the utmost Facility and Readiness; though many of the Boards and Posts were quite dry, and as smooth as a common Plane had left them.

I observed, that at first they thrust their Heads, and about half their Bodies, out of the Water, and held them up against the Wood-work for some time: I imagine, until they found the glutinous Matter, ~~which is constantly about their Bodies,~~ become sufficiently thick or viscid, by being exposed to the Air, to sustain their Weight: Then would they begin to ascend directly upwards, with as much Ease, seemingly, as if they had been sliding along the level Ground; and thus they continued to do, until they they had got into the Dam above.

XI. *A Dissertation on those fossil figured Stones called Belemnites; communicated in a Letter from Mr. Emanuel Mendez da Costa to Martin Folkes, Esq; Pr. R. S.*

S I R,

Read Jan. 29.
1746-7.

THE Origin and Nature of the *Belemnites* having lately been greatly controverted, I have taken the Liberty to address to you the following Thoughts on that Subject; and beg, Sir, if you think them worthy your Regard, you would lay them before the *Royal Society*, as a due Testimony of the great Respect I have for that Learned and Illustrious Body.

The *Belemnites* is a Fossil of different Magnitudes and Colours, ever regular in Shape, which is either cylindric, conic, or thereunto approaching. Numbers of them have, on one Side only, a Chap or Seam running their whole Length; others have it in Part; and in others it is not at all to be observed: It consists of a talse Matter, with an Intermixture of Spar or Crystal, disposed in *Striae* from or near its Centre to its Circumference, and is made up of Crusts inclosing each other, the innermost whereof is as regular as the outermost. Sometimes, tho' seldom, in comparison to the Numbers of the *Belemnites*, in the Centre is a Cavity ever conic, whatever the external Shape of the *Belemnites* be. This conic Cavity is at different times empty, or else filled, either with a solid Body of mineral Matter, Crystal, Stone, *Pyrites*, &c. or with

with a regular jointed conic Body, called by Lithologist the *Alveolus* of the *Belemnites*; which, tho' constantly regular and jointed, is nevertheless found composed of various mineral or metallic Substances.

The *Alveolus* above-mention'd, tho' not fully proved such, yet seems, by the Assent of most of the present Naturalists, to be a Body of marine Origin; a Shell the nighest related to the *Nautilus* Kind: It is concamerated, and even in some is discover'd another great Characteristic of the *Nautilus* Kind, I mean the Gut or *Siphunculus*. Therefore, taking this Body for granted to be of marine Origin (for what Reasons, or of what Kind, is not my present intended Subject to prove) it remains to discuss, Whether this Body became accidentally lodged in the *Belemnites*? or, Whether the *Belemnites* itself is also of marine Origin, and a Part dependent on its *Alveolus*?

Various have been the Opinions of Lithologists concerning the Origin of the *Belemnites*; some have even asserted them of the Vegetable Kingdom; others, that they are Teeth or Horns of Fish, Appendages of Shells, Bodies cast in Shells of the *Tubuli* Kind, or the very Shells themselves, Spines of *Echini*, or a kind of strait *Nautilus*. The three last Opinions are what I shall strive to confute, as they seem somewhat probable, and are now the most prevailing; and prove the *Belemnites* to be a natural Fossil or *Lapis sui generis*. I desire no Recourse to the Sub-~~stances~~ ^{stances} used by others, but hope, Sir, you will agree with me in the Axiom, that all *Belemnites* are of one and the same Origin.

That

That the *Belemnites* are not Teeth or Horns of Fish, I shall refer you to the Letter your late learned Member Dr. *John Woodward* wrote on that Subject to Mr. *Bourguet*, of *Switzerland*, wherein he fully proves the Erroneousness of those Opinions. But a further Argument against their being Teeth, which that learned Naturalist has not touch'd upon, is, that no *Belemnites* have that natural Varnish or Polish, which always covers the Teeth of all Animals; whereas the greatest Part of those fossil Bodies, which we know to be such, as the *Busonitæ*, *Glossopetra*, &c. are found with that same Varnish or Polish. As for their owing their Form to being moulded in Shells, it will appear contradictory to Reason, when we consider, 1°. Their Constitution to be ever as regular as their Figure; and, 2°. That their inner Layer or *Nucleus* is as equally regular as the outer Crust or whole Body; which Particular could never have happen'd, had they been moulded in Shells; as is evident, by the *Turbinitæ*, *Conchitæ*, and other Bodies, which owe their Figures to that Cause. That the *Belemnites* are not Spines of *Echini*, let us first consider, that no Kinds hitherto discover'd have been ever found to have Spines analogous to these Bodies; nor indeed has any marine Shell whatever such a Texture. The immediate Subterfuge for an Answer to this Objection is, that the Kinds of Shells unknown to Mankind are far more in Number than those yet discover'd. I allow it; but think that cannot be an Argument in the present Case, since no one single Species is yet discover'd with such, nor even any Genus, which have Spines analogous to the *Belemnites*.

nites. Nature bears an Analogy through all her Works; and though all the Species of any one *Genus* is not known to any Man, yet that Analogy nevertheless capacitates us to judge of those undiscover'd by those we know. Thus we find of the *Echinus* Kind, all the Species now known are ever found near the Shores; consequently, are not subject to be eternally hidden from us; as is undoubtedly the Case of the *Cornua Ammonis*, and *Conchæ anomie*. They are no pelagian Shells, as those are; Bays and Harbours are the Places where they are fish'd; their Structure even evinces the Reasons for it. We may therefore with Probability conclude, that all the *Echinus* Kind are of the same Nature, and have the same Way of living; that they only inhabit such Places, and that none are pelagian Shells; consequently might have been discover'd.

I am sensible there are some Species of fossil *Echini*; as, the most common *cunoid* or *pilulated Echini*, the common *Echini galeati*, the *Echini clypeati*, and some Kinds of the *Echini ovarii*, &c. which tho' we are certain that they have been marine Shells, yet those particular Species are not known in the Sea: But then several other Species of that same *Genus* are. The Case of this is quite different, since not one single Species of such a *Genus* has ever been found.

The excessive Bigness and Thickness of Numbers of *Belemnites* described by Authors, *viz.* of near two Feet in Length, and above two Inches in Diameter in the thickest Part, others of three Feet long, and others as thick and long as a Man's Arm; not to enumerate those only under a Foot Length, and of proportionable

proportionable Thicknesses, concludes *Echini* of a vast Bigness, to have a Number of such Spines to move.

The Varieties of the *Belemnites*, how can they quadrate to the Spines of one *Genus* of *Echini* only? Solid *Belemnites*, *Belemnites* with a single Crust, or like a Tube, with a conic Cavity only; that empty, or otherwise filled with a solid Mass, or with a regular jointed Body, as the *Alveolus* — *Belemnites* of various Magnitudes and Thicknesses, &c. can all these Varieties be imagined to belong to one *Genus* of Shells, which we suppose to exist to maintain a favourite System?

The Number of Species of *Echini* discover'd are numerous; and the Spines of all those agree in having a hollow *Axis*, which runs proportionably from their *Basis* to their *Apex*, quite different to the *Belemnites*: And for their Constitution, a foreign Naturalist, a Member of the *Royal Society*, Mr. *Klein* of *Dantzick*, who has professedly wrote on this Subject, could only find of two Kinds, *viz.* those of a porous Constitution, which he observed only to belong to one *Genus*; and those of a solid shattery Substance, like a talcy Spar not striated; which is the most general, and is exactly the same Constitution as all the fossil Spines, or *Lapides Judaici* are.

Further, the *Lapides Judaici* have, at some times, been found adhering to their *Papillæ* or Tubercles, and with Fragments of their Shells; whereas no Naturalist has ever known to be found fossil either the Shells, or the Fragments of such a *Genus* of *Echinus*; not even any Remains proportionable to

such large Spines.—In whatever manner the greater Part of such Shells may have perish'd (which is unlikely, if we consider their Texture and Strength), some must have escaped, when the Spines are found in such excessive Numbers every-where, and always perfect and regular; whereas the fossil Spines, or *Lapides Judaici*, as they are call'd, as likewise the *Echini* or Shells, and all the fossil Bodies of marine Origin, are found broken and shatter'd in all kinds of Manners.

As for their being Shells of the *Tubuli* Kind, my Reasons against it are; Were the *Belemnites* such, they must be all tubular more or less; or otherwise must have suffer'd some Degree of Petrification to fill up their Cavities. The Unreasonableness of that Argument is demonstrated by all *Belemnites* being of one and the same Texture and Constitution, tho' Numbers are solid, and Numbers are tubular, in different Degrees. Now one Kind of Petrification, or any other Change in the Earth, which they might have undergone, could never have given so regular a Texture and Substance, and cause such different Effects as Solidity and Tubularity. And if, on the other hand, we allow it to be inconsistent, as it is, to form the Idea of a Shell of the *Tubulus* Kind, by a solid Body, without that Body having suffer'd some Change in the Earth, while buried in it, we must either deny all solid *Belemnites* to be such *Tubuli*, and run to Subterfuges, by owning them to be natural Fossils; or else allow a great Inconsistency, to uphold a wrong System.

That the *Belemnites* are not a tubular Case, which is Part of, and covers a Shell of the *Nautilus* Kind,

as is its *Alveolus*. The Variety of Circumstances already alleged of the *Belemnites* serve to demonstrate the Improbability also of this Opinion, as it has done of the other two. The Numbers of *Belemnites* of all kinds, so plentiful every-where, and the Consideration of how few are furnish'd with *Alveoli*.

Numbers, I am sensible, have conic Cavities; but that those Cavities never did contain *Alveoli*, is evident; that the Sides of the said Cavities are even, and without any circular or other Impressions, which a *Belemnites* that has ever contained an *Alveolus* must have; that Body being in close Contact to all Parts of the investient *Belemnites*, must consequently impress it with its Concamerations; which Impressions must be therefore found on the Sides of the Cavities of all *Belemnites* which ever contained them.

As for asserting, that all the *Alveoli*, which are now found loose, were originally lodged in *Belemnites*, it cannot be; without inferring also, that all *Belemnites* which are now devoid of *Alveoli*, contained such formerly; which, by some external or other Agent, have been forced out and loosened from them.

To consider such an Agent, we must also conclude its Force to have been exceeding great, to loosen out the *Nucleus* of a Body in close Contact with all its investient Parts; and strengthen'd further to it by Ridges and Grooves, such a Force must have compress'd, shatter'd, and otherwise broken and destroy'd the *Belemnites* that contain'd them; which is contrary to Observation. Further, forcing out the *Alveolus* might perhaps easily have happened to the conic *Belemnites*; which hath a Basis of a larger Diameter

Diameter than the Middle, where the *Alveolus* is lodged; but we cannot conceive the same by the cylindric, tuffiform, and other *Belemnites*, of which the two Ends or Extremes terminate pointed; while the Middle, where the *Alveolus* is lodged, is thick and swell'd.

To force an *Alveolus* out of such shaped *Belemnites*, it is evident, that the narrow Ends of the said *Belemnites* must be quite forced open, broken, and shatter'd, before a broader and more capacious Body could be forced through, especially to such a brittle shattery Fossil as the *Belemnites* is. The evident Facts to the contrary of this, are also too common to insist on, since all these *Belemnites* are ever found regular, perfect, and intire.

Further, let us consider the *Alveoli* which are now found in *Belemnites*, they are very seldom if ever found as mere Shells, but ever differently changed or petrified. They are moulded of Stone, *Pyrites*, Crystal, &c. Now it can never be argued, that the contained Bodies can ever be so differently changed or petrified in their Covers or Shells, and those Covers or Shells which admitted such different petrifying Particles to undergo no Change or Petrification whatsoever.

Another Proof against this Opinion, is the diverse Forms of *Alveoli* now discover'd by Naturalists, as conic, cylindric, curved, spiral at the *Apex*, &c. whereas all *Belemnites* which have Cavities have none but conic ones.

These cylindric, &c. *Alveoli* are now found in *Pomerellia* in *Poland*, in the Marble of the Island of *Oeland* in the *Baltick Sea* belonging to *Sweden*, and in the Marble of *Sweden*; in *Gotland*, in
Masses

Masses of Building-Stone; in *Ingria*, in several Parts of *Prussia*, &c. and are commonly of an immense Bigness, to several Feet in Length, and proportionably thick, yet not perfect. For such *Alveoli*, which are only *Nuclei*, we must suppose immense large *Belemnites*; and such we have never heard of, so with Probability we may conclude none such to exist.

I do not doubt the Growth of this Error, of the *Belemnites* being a Part of its *Alveolus*, to have been caused by too rash Conclusions, and too little an Insight into the fossil Kingdom; which has propagated that Assertion of the *Alveoli* being found only in the *Belemnites*; which Experience daily contradicts, since we find them loose, as well as imbedded in many other fossil Substances, as in Marble, Stone, &c. as has been above observed.

These are the Arguments which I allege, Sir, for the Improbability of the said Opinions. I could advance a Number of other Proofs; but as I have already extended my Letter beyond a due Length, I beg Leave, before I conclude, only to offer some few Reasons for their being a natural Fossil, or *Lapis sui generis*.

The very View of a *Belemnites* sufficiently evinces its mineral Origin, and shews it evidently composed of two fossil Substances, a Talc, and a Spar, or bastard Crystal; whereof the former is the Basis, and from which Principle I do not hesitate to attribute its striated Texture. Most of the talcy Bodies are of a fibrous Nature, and several are composed of Crusts inclosing each other, in the same Manner as the *Septa* of the *Lodus Helmontii*, some of the *Asbestos* kind, the *Hematites* Crusts, &c. Of the *Stalactites*

Stalactites Tribe there are several, which so intirely approach the Texture and Constitution of the *Belemnites*, that were their Shapes a little more regular, the most experienced Lithologist might easily be deceived: And I remember, when abroad, to have seen such, of a prodigious Bigness, which, tho' I was then somewhat conversant in the fossil Study, I could not help taking for *Belemnites*. I do not therefore wonder, that *Petrus Assaltus*, in *notis ad Metallothecam Mercati*, p. 282, and *Langius*, *Hist. Lap. figurat. Helvetiæ*, p. 133. should judge them a native figured Fossil, formed in the Earth, of the *Stalactites* kind, if that Term for the *Belemnites* might with Propriety be used.

The Cavities of *Stalactites* in some measure illustrate, and are adequate to the Cavities of *Belemnites*; they are placed in as various Positions, and are only different from them by not being exactly conic. As for the regular Figure of the *Belemnites* being excepted against, I believe few Fossilists will argument that, when we see as perfect regular Figures in the fossil Kingdom as in any other Parts of the Creation; as witness the Salts and Crystals of all Kinds; the rhomboid, hexagonal, columnar, and other *Selenites*; the cubic, octangular, dodecaedral, and other *Pyrites*; the quadrangular Pyramids of Tin, the Rhombs of Iron, Cubes of Lead, and infinite other native Fossils, which would take up Time to enumerate, and which are far more perfect Figures than the *Belemnites* are. Chymical and other Trials and Tests (which I hope to have the Honour to lay before you in some future Letter) demonstrate a greater Certainty of its mineral Origin.

As for that marine Body the *Alveolus*, I cannot think otherwise than that it is of the *Nautilus* Kind; and which, at the Concretion or Formation of the *Belemnites*, became accidentally lodged in its Cavity, in the same Manner as all other marine Bodies became lodged in the various fossil Substances we now find them in.

I beg, Sir, to remark, that the searching into the Origin of this Body is not merely curious, but of great Use; since, if its Origin is fully asserted, we are then better capacitated to search into the Properties and Uses of this Stone, which I do not doubt are many, and which we could never have attain'd to, without first having examined this principal Part of its History. I am,

S I R,

London, Dec. 27.
1746.

Your most obedient,

humble Servant,

Emanuel Mendez Da Costa.

XII. *A Letter from Richard Brocklesby M. D. and F. R. S. to the President, concerning the Indian Poison, sent over from M. de la Condamine, Member of the Royal Academy of Sciences at Paris.*

Honoured Sir,

Read Feb. 5. **T**HE Subjects of Natural History are
1746 7. often strange and uncommon; but

the Authors who have treated on them have not failed, on their Parts, to support and raise the Wonder, and once conceived Astonishment, by ascribing Properties which never existed in Nature; thus indulging the Humour of finding a Marvellous in all Things, Truths have been greatly obscured, and Errors propagated without Number.

It is to this Cause originally (if I mistake not) we are to ascribe the prodigious Multiplicity of Poisons, and that equally numerous Tribe of Antidotes, treated of by the Ancients in their *Materia medica*; and I should be very glad to have found modern Authors always just to Truth, in the Qualities by them ascrib'd to particular Drugs.

Upon hearing lately ~~Paris~~ of Mr. *Juan Antonio de Loa's* Letter to you, Sir, I was suspicious Mr. *de la Condamine* had taken some Facts there upon the Authority of others, or else had been himself a little too much addicted to that general Byas of Mankind, the Love of Prodigy and Wonder,

In

In order to be better satisfy'd, I dissolved, in a certain Quantity of fair Water, as much of the *Indian* Poison as could be suspended, and let it stand to clear 24 Hours; and, having made a superficial Incision with a Lancet into the Nose of a young Cat, a few Drops were sprinkled on the Wound. The Creature at first discover'd no Marks of Injury receiv'd; yet in half an Hour she seem'd, by mewing more than before, to be sensible of some Pain. Thus she remain'd about 20 Minutes; when at length she shiver'd, was sleepy, soon became convuls'd, and, in about half an Hour, her Limbs were flaccid, and her Belly swell'd. These Symptoms continu'd, till she in a short time expir'd.

Some time pass'd, e'er I sat down to inquire what visible Effects had been produced on the Body. I then separated the Head from its Trunk, and carefully examin'd the Brain, and particularly the Origin of the Nerves; but when I had consider'd it thoroughly, I could not discover any preternatural Appearance in any of these Parts. Having spent near half an Hour in this Inquiry, I open'd the *Thorax*, and, with some Surprise, found the Pulsation of the Heart as regular, as if the Animal were in perfect Health. This Appearance continu'd above two Hours after the Cat's Head was off; but afterwards languish'd, and was much weaker.

I then open'd one Ventricle of the Heart, in which the Blood was somewhat coagulated. This may be thought to be partly owing to the Medicine; for, soon after it had produc'd Convulsions in the Creature, I had a Mind to see what Bleeding would do,

and with that View cut off the Tail; but, contrary to my Expectation, the Arteries that supply it with Blood bled very little; and, upon cutting off the Head, the Carotids and both Vertebrales did not pour out above half a common Spoonful.

But as it might be question'd by some, from the Continuance of the Heart's Pulsation, whether the Cat might not possibly, if let alone, have recover'd, I pour'd a few Drops of the same Solution as before into a superficial Wound of a young Dog, weighing 12 Pounds: The Creature, in less than an Hour, shiver'd, became sleepy, was very cold, and so stupid, that he suffer'd himself to be often burnt by the hot Ashes beneath the Grate, where he lay for Warmth.

In this comatous Way he continu'd near four Hours, and then shook off his *Stupor*, and was much better. I left him all Night, and found him next Morning quite well, and as hungry as ever. Upon this I made an Incision at that time into one of the crural Veins, and pour'd a few Drops of the Solution into it: In less than 10 Minutes the Dog gave Signs of great Pain, soon shiver'd, grew cold, was convuls'd, and in less than 20 Minutes died.

Upon opening him nothing uncommon was found, nor was the Blood in this Creature's Heart so thick as in the former. The crural Vein did not bleed from a large Orifice, after the Poison was infused, though it was likely to do it before.

But, as some Authors have said, that Birds in particular are instantly deprived of Life, if the least Particle

Particle of certain Poisons are infused into the Blood, I had a Mind to try one Experiment, and to this End infused a few Drops of our Solution into a cuticular Wound of a small Bird. This occasion'd hanging of the Feathers, and a *Stupor*, in less than 10 Minutes, and kill'd him in somewhat more than fifteen.

I gave about two Drachms of Sugar to another Bird of the same kind, and shortly afterwards pour'd a little of the Solution into its Mouth; but two Drops had scarce touch'd his Tongue before the Creature was convuls'd, and I could with Difficulty lay him down before all Motion was taken away.

I gave these two Birds to two Cats; and whether from eating them or not I don't pretend to say, the Cats made so uncommon a Noise the whole Night, that they disturb'd the Family's Rest.

From these Experiments we find that the supposed Specific is of no manner of Use, even when the Poison is only taken at the Mouth; and from them it may appear probable, that our Poison is nearly upon the same Footing with white Arsenic in the Cure of the Tooth ach.

Thus, Sir, having satisfied myself, I thought I could do no less than give you an Account of the Result of my Trials. If they contain'd any thing that could afford you any Pleasure, ten times the Trouble I have taken would be amply repaid; but as the Subject itself is far from the most entertaining, and I am conscious that others may have carried it on to much better Purpose, so I have nothing to plead

in Excuse of this Trouble, farther than that I have
the Honour to be with great Respect,

S I R,

London, Jan. 14.
1746-7.

Your most obliged,
and humble Servant,

Richard Brocklesby.

XIII. *A Letter from Mr. Richard Dunthorne,
to the Rev. Mr. Cha. Mason, F. R. S. and
Woodwardian Professor of Nat. Hist. at
Cambridge, concerning the Moon's Motions.*

S I R,

Cambridge, Nov. 4. 1746.

Read Feb 5.
1746 7.

IN the Preface to my lunar Tables, I
hinted, that one Use of publishing
those Tables would be, the assisting of Persons desirous
farther to rectify the lunar Astronomy, by enabling
them more readily to compare the *Newtonian* Theory
with Observations.

Since the Publishing those Tables, I have spent
some Time myself in that Comparison; and here
send you the Result, that you may communicate it
to the *Royal Society*, if you think it deserves to be
made public.

As the Motion of every secondary Planet must
partake of the Errors in the Theory of its primary,
I thought proper, before I undertook the Examina-
tion of the lunar Numbers, to compare those of the
Sun with Observations, I compared several Sets of
Mr.

Mr. *Flamsteed's* Observations, after the Method he himself teaches, in *Prolegom. Hist. Cœlest.* p. 133, & seq. which, for many Reasons, I think the best Method hitherto used; and, with the Concurrence of a Gentleman well skilled in these Matters, determined the mean Motion of the Sun at *Greenwich*, the last Day of *December* at Noon, *Anno* 1700, O. S. ϖ $20^{\circ} 43' 40''$ of its Apogee, \oplus $7^{\circ} 30' 0''$, and the greatest Equation of the Sun's Centre $1^{\circ} 55' 40''$; which, I am tully persuaded, are very near the Truth.

The Theory of the Sun being thus settled, I proceeded to examine the Elements of the lunar Astronomy. I began with Observations of lunar Eclipses about the Equinoxes, when the Apogee of the Moon was in the Sun's Quadratures; because at those Times I could conceive the Moon's Motion affected with no Inequality, but the annual one, called by *Newton* the first Equation, and the elliptic one, called *Prosthaphæresis*: From a Comparison of such Observations I obtained the Moon's mean Longitude, which came out $1'$, at least, greater than in the Tables, and very nearly as *Newton* has it in the last Edition of his *Principia*.

I went on to examine the Place and Motion of the Apogee, and Theory of the Increase and Decrease of the Eccentricity, as well as the greatest and least Eccentricities themselves (from the best Observations, and best situate that I could procure) all which agreed so well with the Tables, about the Sun's mean Distances, that I dare venture to make no Alteration therein: Indeed I think the 6th Equation does not so well account for the Variation of the Motion of the Apogee, and Change of the Eccentricity.

tricity, according to the greater or lesser Distance of the Sun from the Earth; and therefore I set myself to compute what Change this Difference of the Sun's Action upon the lunar Orbit would introduce in the Moon's Place in every Situation of the Sun and lunar Orbit; and found, after many tedious Computations, that the Sun being in Apogee, this Change, where greatest, would amount to about $4'$, and to $4' 16''$, when the Sun is in Perigee. In other Distances of the Sun from the Earth, this greatest Change is proportional to the Difference of the Cubes of the mean and present Distances; and in every Situation of the Moon, and of her Orbit, the present is to the greatest Equation nearly as the Sine of the Excess of the Moon's mean Anomaly above twice the annual Argument to *Radius*. It increases the Moon's Longitude, when the Sun is in his

$\left\{ \begin{array}{l} \text{Apogee} \\ \text{Perigee} \end{array} \right\}$ Semicircle, and that Excess $\left\{ \begin{array}{l} \text{less} \\ \text{greater} \end{array} \right\}$ than 180° ; and diminishes it when otherwise *.

In fine, I compared the Theory of the Moon, as to her Longitude, with several Observations, as well in the Octants and Semi-Octants, as in the Syzygies and Quadratures, and found such an Agreement when the above Corrections were made, as seemed rather to be wished than hoped for, considering the many Inequalities wherewith the Sun's Action disturbs the Motion

* If this Equation be increased and diminished in a direct Ratio of the Moon's horizontal Parallax, it will become more exact. And I think, if it were always diminished by a fourth or perhaps a third Part, it would agree better with Observations.

Motion of the Moon, and the Defects to which the best Observations I have hitherto met withal are liable.

I have compared 100 observed Longitudes of the Moon with the Tables; viz. 25 Eclipses of the Moon, all, except the first, taken from *Flamsteed's Historia Cœlestis*, the *Philosophical Transactions*, and the *Memoirs* of the *Royal Academy of Sciences*; the two great Eclipses of the Sun in 1706 and 1715; 25 select Places of the Moon from *Flamsteed's Historia Cœlestis*, and 48 of those Longitudes of the Moon computed from *Flamsteed's* Observations by Dr. *Halley* (as I suppose) printed in the first Edition of the *Historia Cœlestis*. They are as follows;

Place, of Ob-
ervation.

A.D.	Apparent Time at Greenwich.	D's true Place Observed		M. An- om	M. An- om	Ann. Appt.	D's Place compared.		Diff. f. om Observat.
		h	m	s	s	s	s	s	
Dantzick Greenwich Paris	Septem. 7	6	21	35	2	20	6	2	0
	Sept. 18	14	36	48	3	1	6	0	0
	Octob. 19	8	21	54	3	1	7	16½	0
	Febr. 11	10	58	52	7	25	6	28½	0
Dantzick Dublin Greenwich	Dec. 11	10	47	6	5	24	1	8	0
	Nov. 30	10	35	30	5	13	11	26½	0
	Nov. 19	11	13	11	5	2	10	12½	0
	Sept. 18	14	30	37	3	1	4	24	0
Paris New England Marfeilles Greenwich	March 14	9	56	30	8	25	9	18	0
	May 6	12	2	0	10	18	3	1½	0
	March 5	7	13	44	8	16	8	27	0
	Decem. 11	18	29	8	5	23	11	24½	0
Paris London Paris	May 0	21	13	57	10	12	6	22	0
	Octob. 10	7	10	0	3	22	5	27	0
	April 5	13	32	0	9	17	11	9	0
	Sept. 18	9	10	25	2	1	2	12	0
Paris London Paris	June 12	7	34	0	6	24	1	17½	0
	Nov. 20	15	27	30	5	3	9	21	0
	April 21	21	11	5	3	6	12½	1	0
	March 15	15	7	4	8	26	8	22½	0
Paris London Paris	August 18	8	22	46	1	29	10	28	0
	June 17	13	46	10	11	25	5	8½	0
	Octob. 20	15	40	40	1	9	0	0	0
	Febr. 2	8	42	55	4	25	13	39	0
Paris London Paris	July 28	13	0	0	10	16	15	28	0
	June 8	13	47	51	8	28	9	58	0
	Nov. 20	0	40	25	2	10	3	54	0
	June 8	13	47	51	8	28	9	58	0

25 Places of the Moon, computed by myself from *Flamsteed's* Observations, compared with the Tables.

A.D.	Apparent Time at Greenwich.		D's true Place observed.			M. An- om ☉		M. An- om ♀		Ann. Arg't.		D's Place computed.			Diff. from Observat.										
			h	m	s	°	'	''	s	°	'	''	s	°	'	''	'	''							
1684	March	13.	8	9	8				2	28	48	40	2	28	50	44	+	2							
1693	March	6.	7	22	48				3	16	43	12	3	16	44	0	+	0							
	Octob.	11.	18	12	34				2	28	34	2	3	28	38	21	+	4							
1694	Febr.	27.	10	29	16				4	27	27	31	8	27	26	48	-	0							
	August	23	11	13	54				11	0	19	11	2	11	0	21	41	+	2						
1694	Septem.	15.	5	34	13				8	27	1	34	2	8	27	1	41	+	0						
	Septem.	21.	10	50	31				11	22	47	41	3	17	11	22	49	16	+	1					
	Decem.	13.	6	2	53				0	6	28	43	5	3	6	29	58	+	1						
1695	Febr.	8.	3	55	22				1	5	14	3	7	22	1	5	12	14	+	1					
	July	9.	5	56	14				7	2	2	42	0	0	2	7	2	3	4	+	0				
	Septem.	8.	8	30	26				10	3	0	12	2	21	10	2	59	50	+	0					
1696	January	16.	17	29	2				7	5	4	20	6	29	2	29	5	0	11	+	1				
	March	4.	9	8	13				4	12	2	24	8	16	11	3	4	12	0	59	+	1			
1697	February	18.	6	29	4				2	20	52	29	8	2	8	19	5	10	2	20	51	7			
	19.	7	22	57	3				3	4	18	16	8	3	9	2	1	5	11	3	4	17	5		
	Septem.	15.	7	54	40				10	1	7	8	2	28	3	21	11	9	10	1	5	38	+	1	
1698	Septem.	8.	11	2	35				11	12	15	6	2	21	3	24	9	23	11	12	15	27	+	0	
	Novem.	27.	3	49	54				10	11	33	39	5	9	2	3	10	3	10	11	31	13	+	2	
1699	March	5.	12	8	23				5	28	15	59	8	16	8	29	1	3	1	5	28	16	11	+	0
1701	Septem.	28.	6	55	23				9	27	13	17	3	10	9	9	3	8	9	27	12	49	+	0	
1702	Octob.	16.	6	16	49				10	3	25	41	3	28	8	11	5	13	10	3	26	27	+	0	
1703	Septem.	13.	11	58	50				11	28	50	30	2	25	8	28	3	31	11	28	53	30	+	3	
	Octob.	6.	6	28	39				9	27	1	44	3	18	7	4	3	23	9	27	0	38	+	1	
1706	Octob.	10.	11	11	10				1	1	11	15	3	22	6	0	11	25	1	1	10	17	+	0	
1714	Septem.	6.	6	34	7				9	3	44	13	2	18	3	17	0	0	9	3	45	24	+	1	

48 Places of the Moon, computed by Dr. Halley, from *Flamsteed's* Observations, compared with the Tables.

A.D.	Apparent Time at Greenwich.	D's true Place observed.			M. An- om. O.		M. An- om. D.		Ann. Argt		D's Place computed			Diff. from Observat.		
		h	'	''	s	''	s	''	s	''	s	o	'		''	
1689	Novem. 16.	11	59	0	4	29	6	1	0	0	2	6	14	40	+	1 57
	Decem. 9.	6	1	0	5	21½	3	21	0	20½	11	28	48	40	+	0 14
	10.	6	46	35	5	22½	4	4	0	21½	0	12	47	14	+	0 25
	12.	8	26	33	5	24½	5	0½	0	23	1	12	10	39	+	2 29
	13.	9	24	30	5	25½	5	14	0	24	1	27	36	16	—	2 20
1690	16.	12	42	0	5	28½	6	24	0	27	3	15	15	48	+	0 54
	4.	3	3	46	6	17	2	24	1	14	11	11	13	8	+	0 9
	6.	4	30	15	6	19	3	20½	1	16	0	8	14	36	+	1 5
	10.	7	59	22	6	23	5	14½	1	19½	2	6	10	37	—	0 43
	12.	10	8	49	6	25	6	121	1	21	3	7	3	59	—	1 22
February	13.	11	14	0	6	26	6	25½	1	22	3	22	35	19	—	1 9
	2.	2	25	39	7	15½	3	5	2	10	0	3	56	9	—	1 15
	5.	4	51	10	7	18½	4	26	2	12½	1	16	34	15	+	2 42
	7.	6	48	17	7	20½	5	24	2	14½	2	15	59	13	+	0 59
	8.	7	51	54	7	21½	6	8	2	15½	3	0	56	31	+	0 17
	10.	9	56	26	7	24	7	6	2	17	4	0	55	1	—	0 23
	11.	10	52	31	7	25	7	20	2	18	4	15	41	49	—	0 27
	14.	13	19	31	7	28	9	2	2	21	5	28	11	44	—	0 56
	19.	17	3	55	8	3	11	12	2	25½	8	2	21	17	—	2 9
	21.	18	45	37	8	5	0	10	2	27½	8	26	59	6	—	2 19

[illegible]

a, The Time of the Middle of this Eclipse here set down is from the Beginning and End; but *Hevelius* says he could not observe the Beginning exactly. Several intermediate *Phases* compared together shew the Middle to have been about 4' sooner; to which the Moon's Place computed is $os. 6^{\circ}. 14'. 3''$. and Diff. $+ 34''$.

b, b, b, The Moon's Places, observed on *Feb. 2. April 7. and May 22.* are computed by myself, from the Observations; there being manifestly Errors, either of the Computation or Press, in those printed in the *Hist. Cælestis*.

Several observed Latitudes of the Moon, which I have compared with the Tables, shew them to be very near the Truth, both in the Motion of the Nodes, and also in the Quantity and Variation of the Inclination. I am,

S I R,

Your humble Servant,

Richard Dunthorne.

XIV. *Extract of a Letter from Mr. Leonard Euler, Prof. Mathem. and Member of the Imperial Society at Petersburg, to the Rev. Mr. Cha. Wetstein, Chaplain and Secretary to His Royal Highness the Prince of Wales, concerning the Discoveries of the Russians on the North-East Coast of Asia.*

Berlin, Dec. 10. 1746.

Read Feb. 5. —
1746-7.

AS you are desirous to hear something more particular concerning the *Russian* Expeditions to the North and North-East of *Asia*, I will here give you an Account of all that has come to my Knowledge relating to the same. But as I should, on the one hand, be very glad that these Observations might give any Light concerning the Passage now sought through *Hudson's Bay*, I should, on the other be very sorry, if Mr. *Behring's* Opinion, who believed that the new Land he had discovered was joined to *California*, should rather lead us to doubt of the Success of that glorious Undertaking. I wish, however, that a happy Experiment may soon inform us certainly of the Truth. In the mean time you will not be sorry to be acquainted with the Reasons upon which Mr. *Behring's* Suspicions were founded, notwithstanding the Objections you have been pleased to make, and to communicate to me upon that Head.

First, 'This new Land, which he fell in with at the Distance of 50 *German* Miles from *Kamschatka* towards the East, was followed by him, and coasted
for

for a great Way, tho' I cannot say how far : From whence alone it will appear, that an Abatement must be made in the Distance of 30 Degrees, or thereabouts, which you suppose to be between the last known Head-Land of *California* towards the West, and the farthest Extremity of this new discovered Land towards the East.

Secondly, Capt. *Behring* having had the Opportunity of observing an Eclipse of the Moon as *Kamschatka*, concluded from the same, that that Place lay much farther off to the East, than is expressed in any Map ; and that, to represent it truly, it ought to be transferred into the other Hemisphere, as its Longitude is more than 180 Degrees [East from the Isle of *Ferro*]. For this Reason Captain *Behring's* new Land will be considerably approached to the last known Part of *California*, and will not indeed appear to be many Degrees from it.

What we have therefore still to hope is only, that in this unknown District there may be found some Streight, by which the *Pacific Sea* may freely communicate with *Hudson's Bay* ; but if it shall appear that there is no such Passage, it must then be concluded, that whatever further Progress may happen to be made through *Hudson's Bay*, the Opening at last must only be into the *Frozen Sea*, from whence there could be no passing into the *Pacific Ocean*, but by the Neighbourhood of *Kamschatka* ; and this Way would without doubt be too long, and too dangerous, to be master'd in the Course of one Summer.

I very much doubt whether the *Russians* will ever publish the Particulars of their Discoveries, either such as have been made from *Kamschatka* towards

wards *America*, or such as have been made upon the Northern Coasts of *Asia*. And indeed it is but very much in general that I know the Success of this last Expedition. What I do was communicated to me by Order of the Court, from the College of Admiralty, for me to make use of it in the Geography of *Russia*, which I was at that time charged with.

They passed along in small Vessels, coasting between *Nova Zemla* and the Continent, at divers times, in the middle of Summer, when those Waters are open. The first Expedition was from the River *Oby*; and at the Approach of Winter the Vessels shelter'd themselves by going up the *Jeniska*; from whence the next Summer they returned to Sea, in order to advance further Eastward; which they did to the Mouth of the *Lena*, into which they again retired for the Winter-Season.

The third Expedition was from this River, to the farthest North-East Cape of *Asia*. But here they lost several of their Boats, and a great Part of their Crew, so as to be disabled from proceeding, and from making the whole Tour, so as to arrive at *Kamschatka*.

It was however thought, that a further Attempt was then unnecessary, because Captain *Behring* had already gone round that Cape, sailing Northward from *Kamschatka*.

The *Russians* have not attempted the Passage round *Nova Zemla*; but as they have passed between that Land and the Coast of *Asia*, and as the *Dutch* did formerly discover the Northern Coasts of *Nova Zemla*, we may now be well assured, that that Country is really an Island.

XV. Abstract of a Letter from Mr. Wm. Arderon, F. R. S. to Mr. Henry Baker, F. R. S. containing some Observations made on the Bansticle, or Pricklebag, alias Prickle-back, and also on Fish in general.

Dear S I R,

Read Feb. 5. 1746-7. **M**ANY of my Leisure Hours last

Summer were employed in attending and making Observations on several Kinds of Fish; some whereof I with great Care have preserved alive in glass Jars for many Months together.

I sent you some time ago a brief Account of what I had observed remarkable in the Dace and Ruff, and am now going to lay before you what I have thought worth Notice in that little common Fish called the Prickle-Back.

About the Beginning of last *April* I took a Bansticle out of our River, full of Spawn, and put it into one of my glass Jars, at the Bottom of which I had placed a small Quantity of Sand, as I always do in every Vessel wherein my Fish are kept; and about the 20th of *May* it buried its Spawn in the said Sand. I was in Hope, the Spawn would have produced a young Brood, but was unluckily disappointed; which I impute to its being frequently disturbed by the pouring in of fresh Water.

For some Days after I had caught this Bansticle, it refused to eat any thing I could offer it, as is common with all Fish I have yet kept; but frequently
giving

giving it fresh Water, and coming often to it, it became so familiar as to eat small Worms I now-and-then threw into the Jar, and from that time grew so tame as to take them out of my Hand; nay it became so bold at last, that when its Belly was full, or it did not like what I offer'd, it would set up its Prickles, and with its utmost Strength made a Stroke at my Fingers, if I put them into the Water to it.

This Fish was of so unfociable a Disposition, that it would suffer no other Fish to live in the Jar with it, and so audacious, as to attack whatever I put in, though ten times its own Size.

One Day, for the sake of Diversion, a Friend being then with me, I put a small Ruff into the Jar to it, which the Bansticle immediately assaulted and put to Flight, having in the Conflict torn off a good Part of its Tail; and would, I dare say, have killed it, had I not separated them very soon.

Infinite Numbers of these Prickle-backs are to be found in almost all fresh Waters, where-ever it is possible for Fish to live; and whatever other Kinds the Water is replenished with, this certainly is one, as far as I have yet had Opportunity to make any Enquiry.

The Endeavours they use, and the Ability they have, to get from Place to Place, are also extraordinary; for though the largest of them scarce measures above two Inches in Length, I have seen some of them leap out of the Water a Foot high perpendicularly, and even much further in an oblique Direction, when they wanted to get over Boards or Stones, or some other Obstacle to their Passage.

It is scarce to be conceived what Damage these little Fish do, and how greatly detrimental they are to the Increase of all the Fish in general amongst whom they inhabit. For it is with the utmost Industry, Sagacity, and Greediness, that they seek out and destroy the Spawn of all Sorts of Fish; and moreover all the young Fry, that come in their Way, are pursued by them with the utmost Eagerness, and swallowed down without Distinction, provided they are not too large.

And in Proof of what I here assert, I must assure you, that the Bansticle before-mention'd in my glass Jar, did, on the 4th of *May* last, devour, in five Hours Time, 74 young Dace, which were about a Quarter of an Inch long, and the Thickness of an Horse-Hair. Two Days after it swallowed 62, and would, I am persuaded, have eat as many every Day, could I have procured them for it.

Could Gentlemen, who take Pleasure in Fish-ponds, intirely prevent these Destroyers from getting into them, I am convinced their Produce would be much greater than it commonly is: And though it may not be possible to keep them out intirely, it is most certainly adviseable to be very diligent in the destroying of them: And whenever, by Netting, or other means, any of them are got out of the Water, never throw them in again, on a Supposition of their being harmless.

Nature has furnished this little Fish with a kind of Breast-plate or Armour, to be its Defence against any outward Injury: She has likewise bestowed upon it several offensive Weapons or Spines, placed upon its Sides and Back, which it immediately
erects

erects upon the least Appearance of Danger, or when it attacks some other Fish. The Sharpness of these Prickles guards it well enough from larger Animals, that might otherwise prey upon it; but neither these, nor all the Endeavours it can use, are able to free it from an Enemy that torments it even to Death; what I mean is a kind of Louse, of an oval Figure, having eight Legs, and a very transparent Body, which is able either to swim or crawl, and sticks on it so fast, sucking and plaguing it all the while, that it makes it almost mad.

One remarkable Particular in this Louse is, that its little fibrillous Fins are always in Motion, whether the Creature be swimming about, or fixed upon the Fish.

ALL Fish regulate their Times of Eating and Abstinence by the Temperature of the Air, and the Quarter from whence the Wind blows; and would those Persons who are Lovers of Angling, take the Pains to keep a few small Fish in Glasses, they might at any time easily foretel, from their taking or refusing Food, what Sport is to be expected, and often save themselves many a weary Step taken to no purpose.

I have always observed, amongst the Fish I keep in Jars, that such as have lived a while together contract so great an Affection for each other, that if they are separated they become melancholy and sullen, and are a long time before they forget the Loss.

About *Christmas* last I put two Ruffs into a Jar of Water, where they lived together until *April*; when, at the Desire of a Friend, I gave one of them
away

away. — After this Separation the Fish that remained with me was so affected, that for three Weeks it would eat nothing I could give it; and therefore, fearing it would pine to Death, I sent it to the Gentleman on whom I had bestowed its Companion; and what is very extraordinary, upon being put together again, it eat immediately, recovered its former Briskness, and both of them are still alive.

I have made abundance of other Observations on Fish, but shall only add at present, that when they remain supine and unactive, they every now-and-then gape and yawn, as most Land-Animals do, when weary of the Situation they are in. I remain,

S I R,

Norwich, July 9.
1746.

Yours, &c.

W. Arderon.

XVI. *A Supposition how the white Matter is produced, which floats about in the Air in Autumn; in a Letter from the same to Mr. Baker.*

S I R,

Norwich, Aug. 28. 1746.

Read Feb. 26.
1746-7.

HAVING lately a large Spider in my Hand, by chance I let it fall, and it hung by its Thread, as they very commonly do. On holding my Hand very still it readily ascended up it again; and thus, by giving it a Shake, and then holding my Hand still, the Spider ascended and descended

scended a great many times. I thought, at first, it had spun a new Thread at every Descent, and was desirous to have measured how long an one I could cause it thus to spin; but, upon a stricter Examination, I very plainly perceived, that whenever it ascended, it wound its Thread with its Feet into a sort of Coil, and when it descended only unravelled it out again

The Manner how they perform this is diverting enough; but as Spiders may be had almost in every Place, and the Experiment is so easily tried, I shall forbear describing it; and only add, that as these Coils of Thread are exactly like those floating in the Air towards the End of Summer, I think it is not improbable those are made in the same Manner, when Spiders have a Mind to direct their Course in the same Direction their Threads lie.

XVII. *Some Remarks on the precious Stone called the Turquoise; by Cromwel Mortimer, Sec. R. S. &c.*

Read Feb. 26. **T**HIS Stone has received its modern
1746-7. Name of *Turchesia*, and *Turquoise*, from its being most commonly brought from *Turky* into various Parts of *Europe*. *De Boodt* * says, the Colour of this Gem is a Variegation of Green, White, and Blue; and that there are two Sorts of it, the oriental, from the *East Indies* and *Persia*, and the
occidental.

* *Gemmar. et Lap. Hist.*

occidental, from *Spain, Germany, Bohemia, Silesia, &c.*; that in *Persia*, where it is found in greatest Plenty, it adheres to black Stones, as if it were an Excrement or a Transudation from them. A Stone of this sort is seldom found to exceed a Walnut in Size; and he mentions one in the Great Duke's *Museum*, on which the Head of *Julius Caesar* is engraved, as a very extraordinary Sample: He adds, That he never saw one bigger than an Hazel-nut; that some of the oriental ones have the Faculty of preserving their Colour perpetually, which are called Stones of the old Rock; and that others lose their Colour gradually, and are called of the new Rock. He then gives an Instance of a Turquoise which had lost its Colour upon being laid by some time after its Owner's Death, which recover'd its beautiful Colour upon our Author's wearing it upon his Finger in a Ring.

Cæsius, in his Treatise *de Mineralibus*, p. 601. says, This Stone is called *Turcois* by *Mylius*, in his *Basilica chemica*; by *Albertus Magnus*, in his Treatise of Minerals; and by *Rueius*, in his Treatise of Gems: but *Turca*, by *Cassinius de Lapillis symbolicis*. *De Boodt*, and *Dr. Woodward*,* with other modern Writers, take it for the *Callais* of *Pliny*. *Salmasius*, in his *Plinian. Exercit.* p. 142. says, Many have mistaken the modern *Turquoise* for the *Cyanus*, but that the *Cyanus* was transparent like the Saphire; whereas the *Turquoise* is a sort of Jasper.

Dr. Woodward, in his Letter to Sir *Jo. Hoskyns*,† says, That the *Turcois*, or *Callais* of *Pliny*, is nothing else, but fossil Ivory tinged with Copper. I do

* Method of Fossils. Letters, p. 17.

† *Philos. Trans.* 16.

do not deny, that some Stones sold for *Turquois*, and possibly all that the Doctor saw were certainly such; but I imagine those which the Authors call of the old Rock, and in which the Colour is permanent, are real mineral Stones: This Sample now before us seems to shew this, from both the Form and Size: Its Shape shews it not to be Part of any animal Bone; but its botryoid Form is to me a Demonstration that it is the Product of Fire, which had once melted this Substance; and that when it cool'd, its Surface was formed into Bubbles and Blisters, in the same manner as the *Hæmatitis botryoides* or Bloodstone, whose Surface consists of Knobs, resembling a Bunch of Grapes.

That the *Elephas* *ἑρμαῖος*, or *Ebur fossile* of *Theophrastus* *, said to be of various Colours, I do not in the least deny to be tintured with Copper, and to be what Dr. *Woodward* calls the *Turquois*: Indeed I suspect it to be what *De Boodt* calls of the new Rock; and says is liable to lose its Colour, which it recovers again from the *Effluvia* of the Person who wears it. I therefore, for Distinction sake, think all these Stones of the Ivory Origin should be called *Pseudo-Turkeshia*, or bastard *Turquois*; and the other Sort, of which this before us is one, the true or real *Turquois*; for, by Examination in the chemical Way, I find it to be a very rich copper Ore; some of it pounded and dissolved in Spirit of Hartshorn gives a deep Blue; in *Aqua fortis* a fine Green; and an iron Wire put into it was in 1 Hour's time incrustated with Copper: Some of it calcined, without any Flux in a Crucible, run

K k k

to

* See *Theophrastus's Hist. of Stones*, translated, &c. *John Hill*, Lond. 1746. 8°. p. 94.

to a Slag, or half vitrified Substance; whereas the same Heat, had it been Ivory or Bone, would have reduced it to a white Ash like Bone-Ashes; for I exposed it to such a Fire as vitrified the Tile that cover'd it. Its Hardness and Consistence to an Engraver's Tool seems to be the same as common white Marble: Its Colour is not mended by Heat, but it grows brittle when red hot.

This Specimen, now shewn to the *Society*, was about 12 Inches long, 5 Inches broad, and in some Places near 2 Inches thick; rough on the under Side, as though broken off from the Rock it had been affixed to; and the upper Side was composed of smooth polished Knobs, in Form like to the botryoid Iron Ore.

Sir *Hans Sloane*, in his noble *Museum*, has several Specimens of these oriental *Turquoises*, all botryoid; especially a Mass from *China*, about three Inches long, two broad, and near an Inch thick: All which seem to be Copper Ores: And he has likewise Samples of *Turquoises* from *Spain*, and the South of *France*; which are all small, and seem really to be Pieces of Ivory tinged with Copper.

XVIII. *A Description of a curious Echinites;* by Mr. Henry Baker, F. R. S.

Read Feb. 26.
1746-7.

MR. Baker takes the Liberty of shewing the *Society* a very extraordinary *Echinites*, the like to which he has never seen in any *Museum*, or found described by any Author. For the *Echinites* usually met with, are made up either of Chalk or Flint, or some stony, chalky, or sparry

sparry Matter, formed within the Shell of the *Echinus*, and taking their Figure thence as in a Mould: Which Shell is oftentimes broken off and gone, but remains at other times impregnated with talcy or sparry Particles: Whereas the Subject now laid before us is composed of a transparent crystalline Substance, which has received its general Figure by having been circumscribed within the Shell of some *Echinus*, and shews linear Ridges and Divisions correspondent to the Lines and Plates found in this kind of *Echinus*.

Was this all,¹ it would be a very uncommon Production, as these Bodies have been very rarely known to be formed of Crystal *; but it is render'd much more curious and extraordinary, by having exact Rows and Series of little Cells, all of the same regular Figure, though lessening gradually in Size, as they ascend from the Base upwards. (*Vide* Fig. 3.

This Body having been formed within the Shell of an *Echinus*, one would expect (as is the Case in all other *Echinites* usually known), that its Figure should be exactly answerable to the Mould wherein it was formed; but Mr. *Baker* begs Leave to take notice, that the *Echinus*' Shell is perfectly smooth internally, having no rising Parts correspondent to these Cells or Cavities; and therefore, as it could not receive its Configuration from thence, it must be owing to the natural Shooting of the crystalline Matter

* Sir *Hans Sloane* has a Mass, which was form'd within an *Echinus*, the Shell being broken off; it is one Half or Side Crystal, the other Side Chalk.

Matter (tho' unlike every thing of that kind yet described), or to some other Cause, which he don't pretend to know †.

The Configuration seems nevertheless in some measure to correspond with the Nature of the Shell wherein it was formed: As to the Number of the Rows of Cells, they being ranged by Fives, as the *Papillæ*, *Indentings*, *Lines*, or other *Marks* on the recent Shells of *Echini* constantly are; these Rows are twenty in Number; viz. five double Ranks of large and extremely regular Cells, as at *aa*, &c. between which lie five other double Rows of smaller and less distinct *Cellulæ*, shewn at *bb*, &c. These Cells, which are hexagonal, and whereof those in every Row lie alternately to those of the next (by which means they fill up the whole Space), decrease in their Size gradually, as they approach nearer to the Top; all the Rows at last almost concentrating at the *Apex*, leaving only a small Space or Vacuity, where in the Shells themselves of this kind of *Echini*s there is an Aperture. The smooth Part at *A* is formed of a pebbly Stone, bearing the same Marks as are usually found in the Impression of these *Echinitsæ* dug up in Gravel-Pits; which proves, that this must have receiv'd its genecal Figure from one of those Shells, whatever has been the Cause of this remarkable Configuration of the crystalline Part.

This curious *Echinite* was found in a Marl-Pit at *Baborough*, about three Miles West of the City of *Norwich*, and presented to Mr. *Baker* by Mr. *Wm. Arden*. F. R. S.

† Perhaps to some Cells or Membranes belonging to the Body of the *Echinus*.



Fig. 1.

p. 534

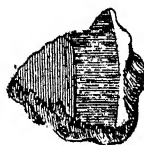


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Fig. 2.



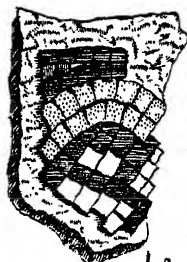
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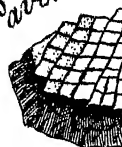
N° 3.

p. 558.

N° 1.



Mosaic Pavement



Fragments of the



Bridlington and Flamboro



7. 3.

535.



Stanford & Burgh

York to



Flue
p. 548.



York.

Roman Road to York

Road to Pocklington

Currently Hill

Tumuli

A Survey of the
Roman Works in Yorkshire
where the Ancient Town of
DELGOVITIA
is Supposed to have stood.

aa aa Vallies markt in the
Original with Green.
bbb b Barriers markt Yellow.
ccc c Watch Guards markt red.

Roman Road to Malton

These Works continued
towards Bridlington

These Vallies continued
towards Bridlington

Roman Road
to Bridlington Bay

Works continued inwards
on the ditches in breadth
about 75 Yards Road



p. 548



Fig 3. p 519

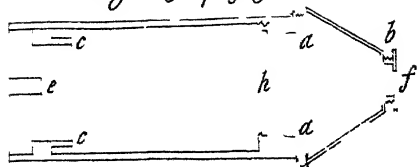


Fig 4 p 519

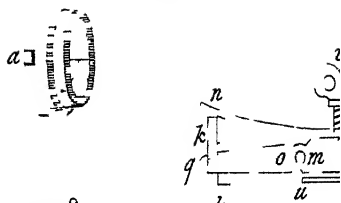


Fig 2 p 518

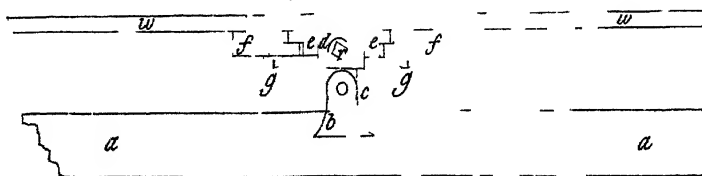


Fig. 1 p 518

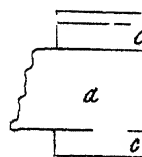
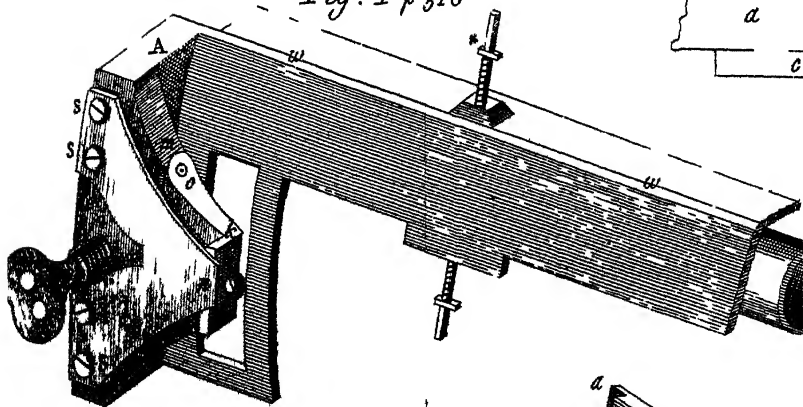
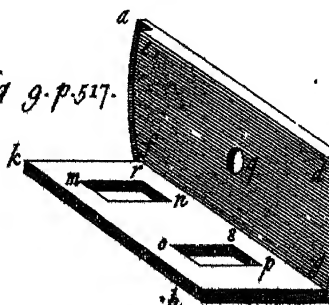
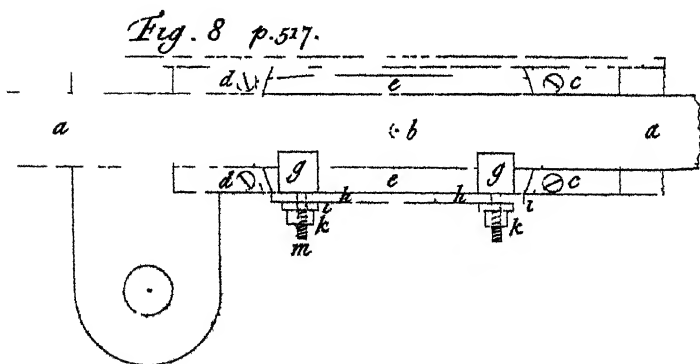
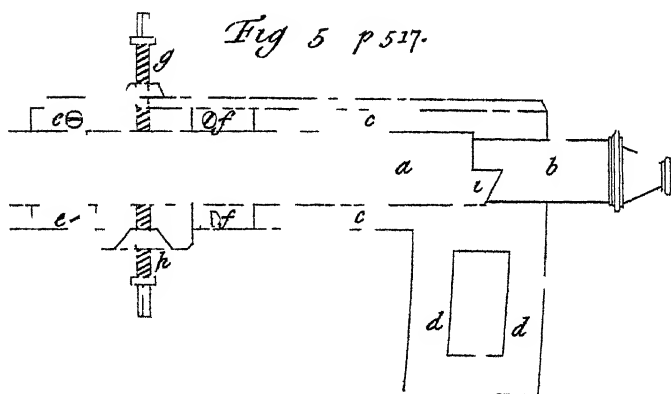
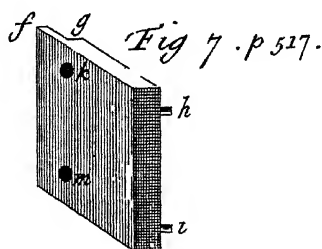
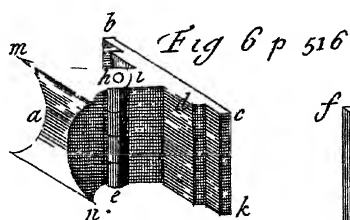
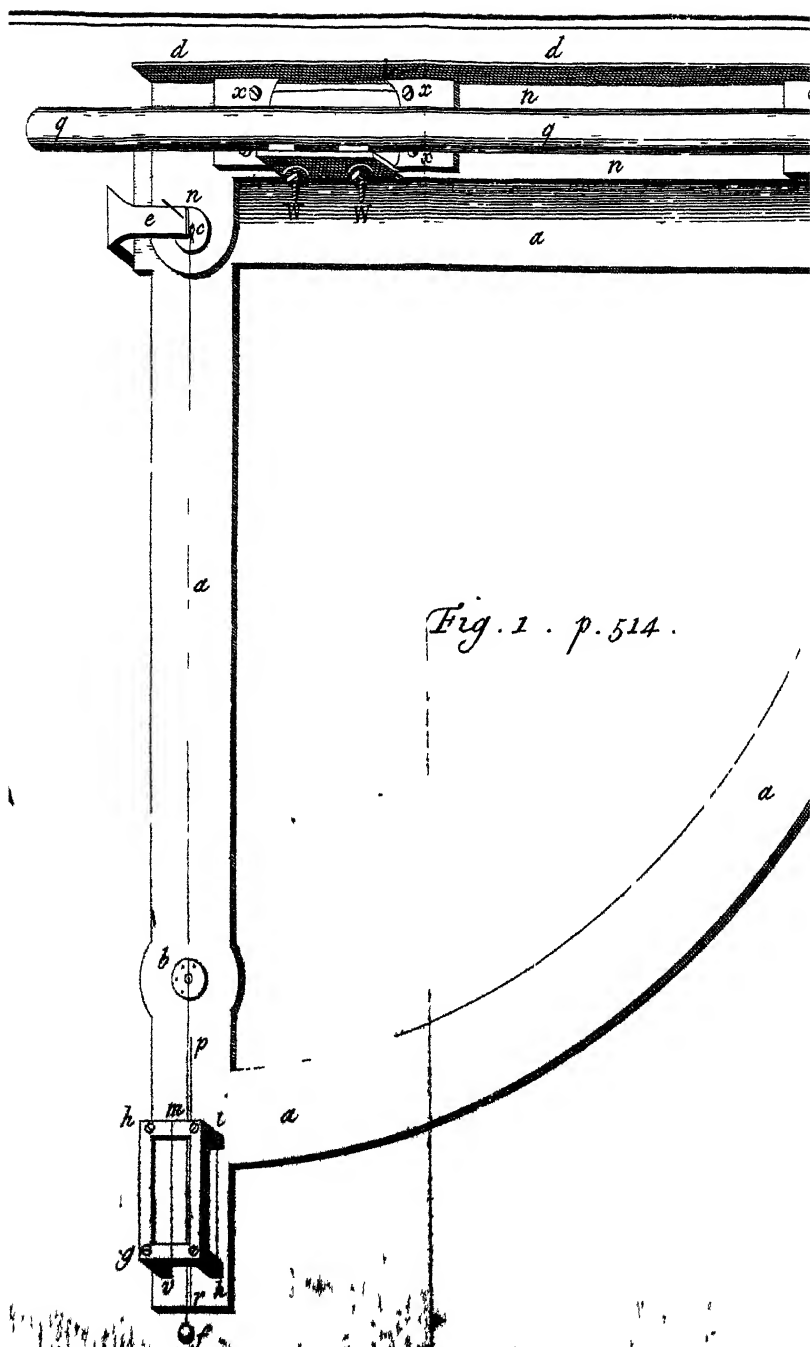


Fig 9. p 517.







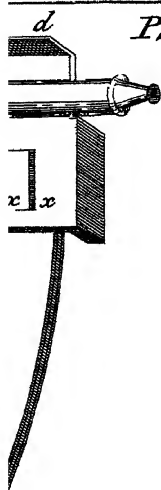


Fig. 2 . p. 513

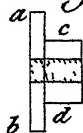


Fig. 3 . p. 520.

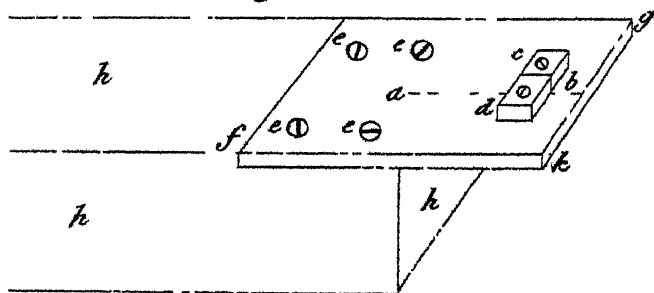




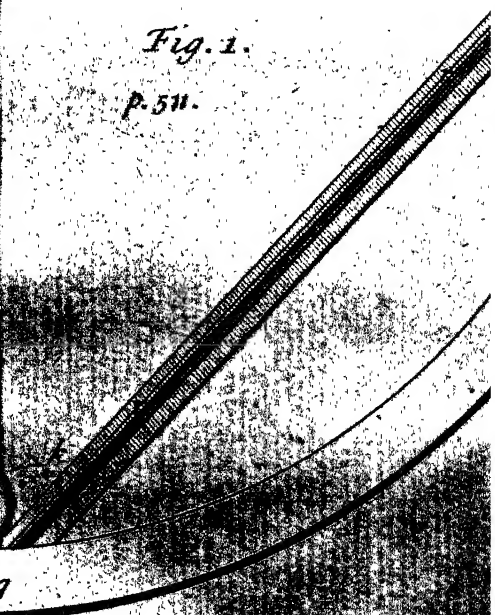
Fig. 1.

p. 511.

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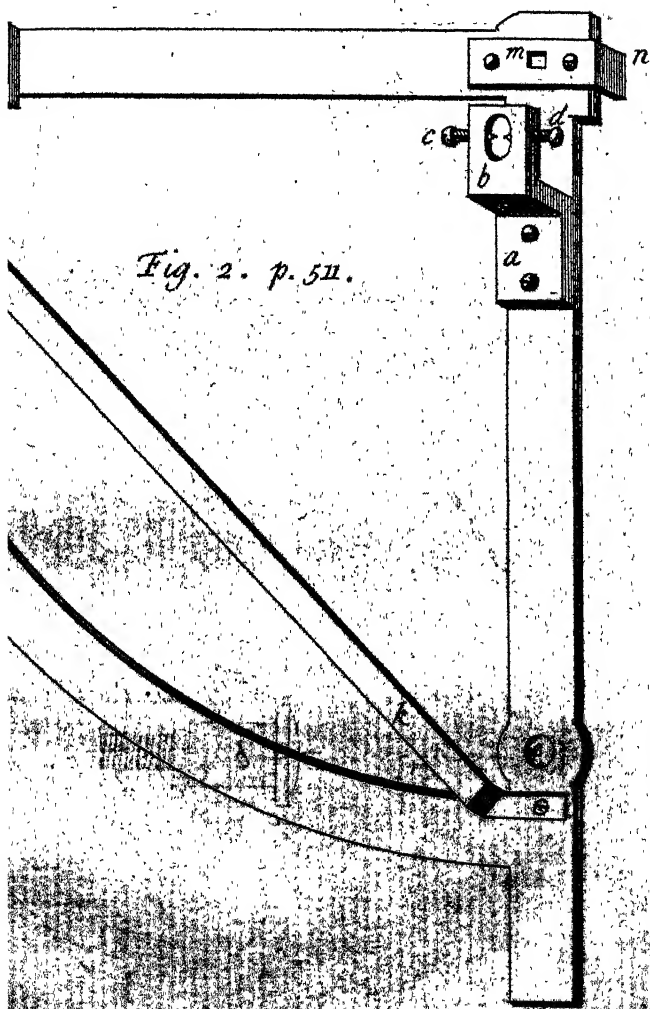


Fig 1 p 508

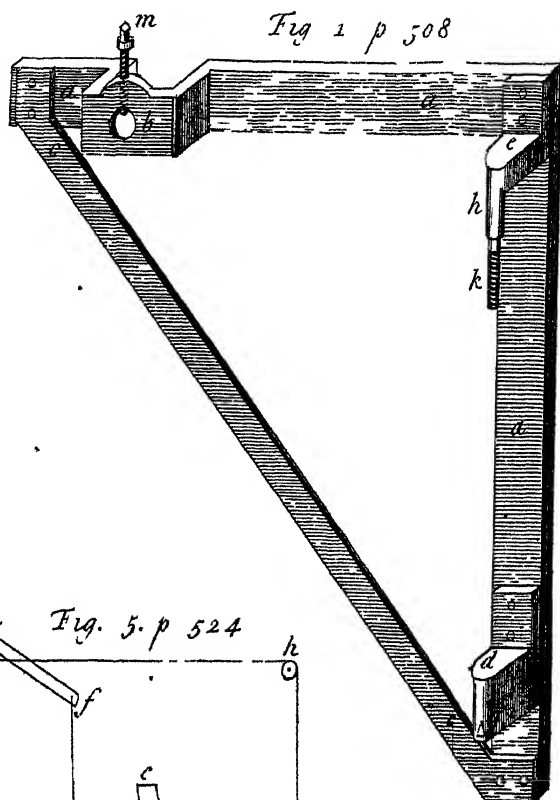


Fig. 5. p 524

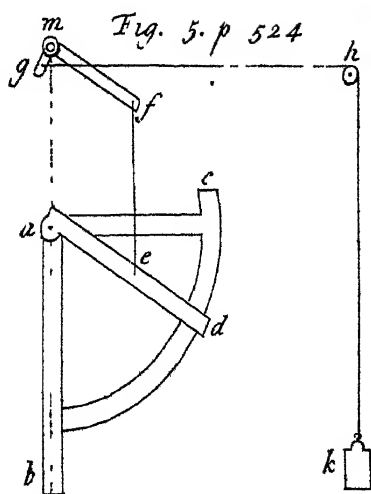


Fig 2 p 508

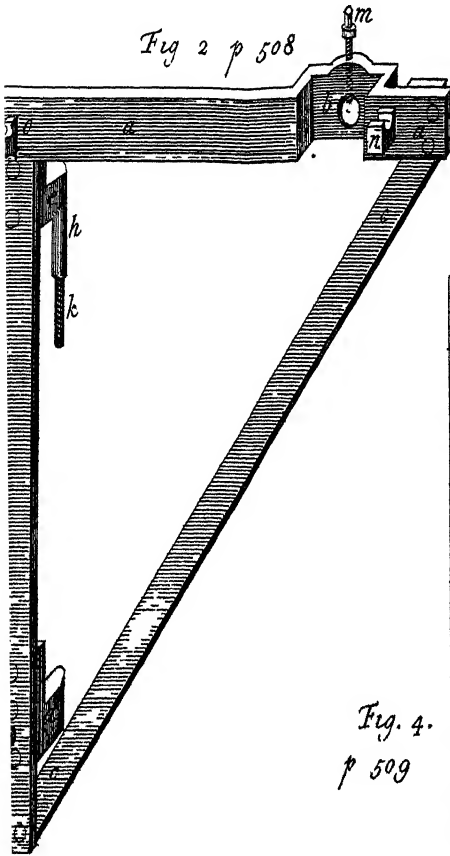


Fig. 3. p. 511.



*Fig. 4.
p 509*

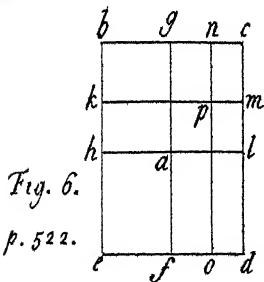
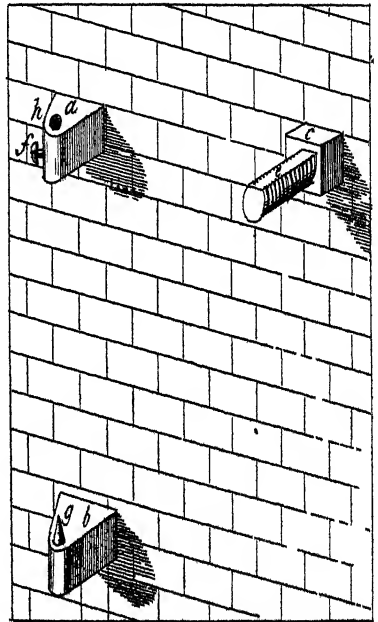


Fig. 4. p. 499.

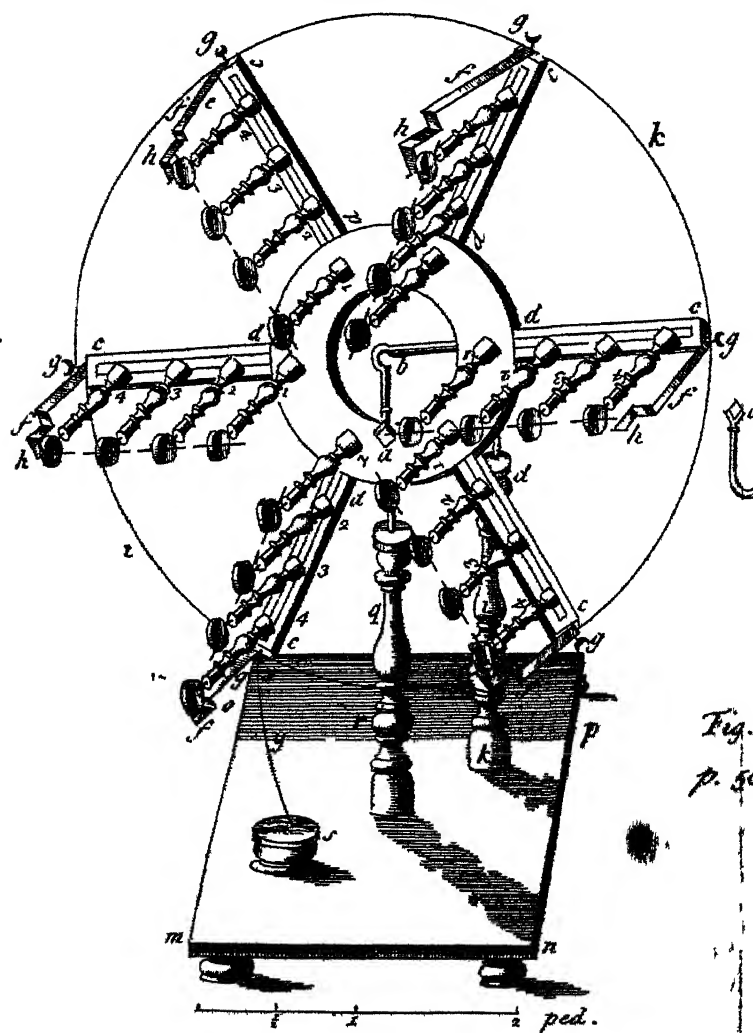


Fig.
p. 50

Fig 2 p 498

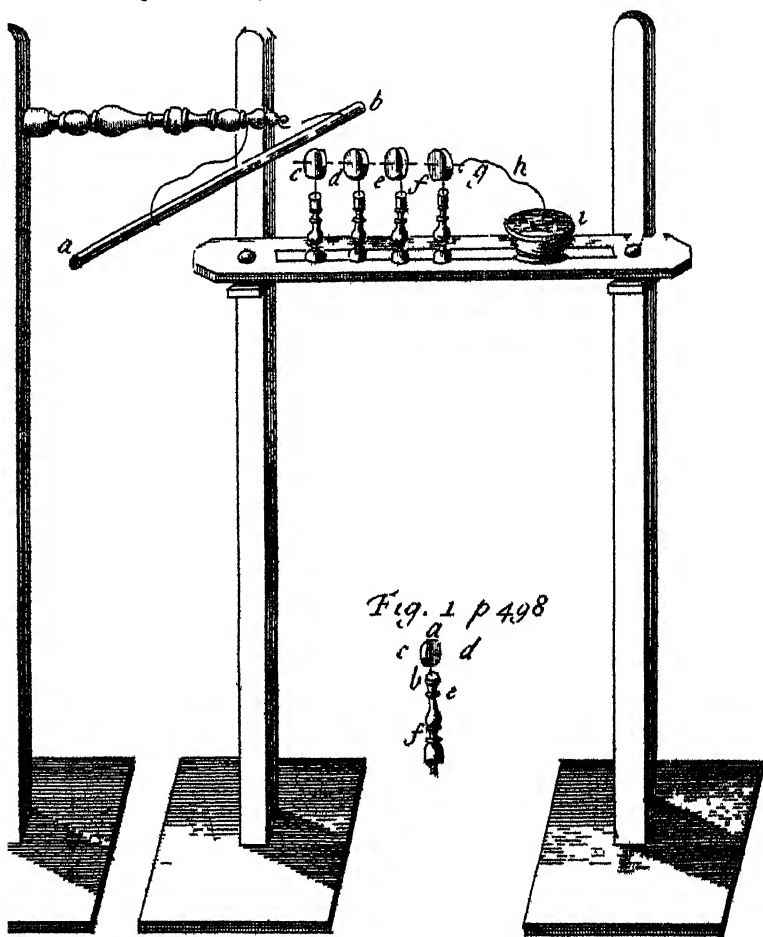


Fig. 1 p 498



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Fig. 1.

p. 46.



Fig. 3.

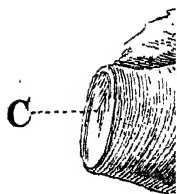
Fig.



Fig. 2.

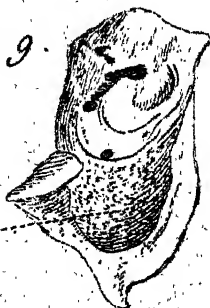


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Fig

Fig. 9.



Fig

Fig. 8.



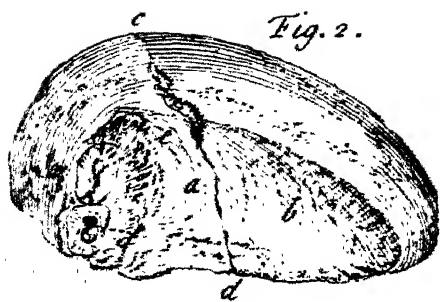


Fig. 2.

p. 454



Fig. 3.

Fig. 1. p. 451

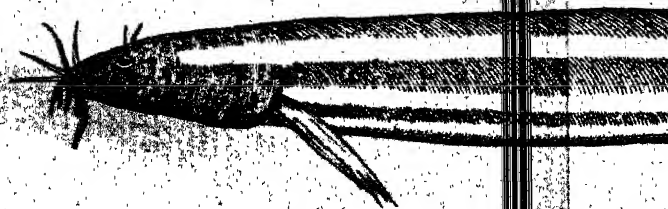


Fig. 8. p. 45

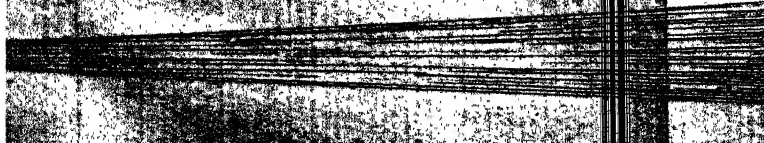
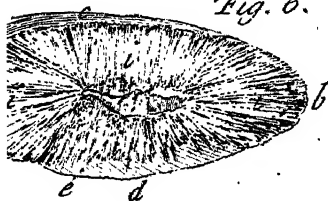


Fig. 6.



d Fig. 7.

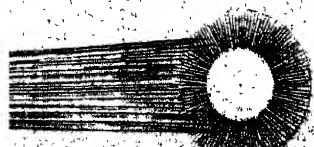
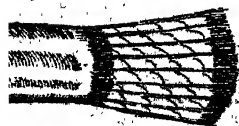
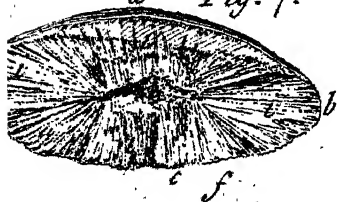


Fig. 1. p. 4.

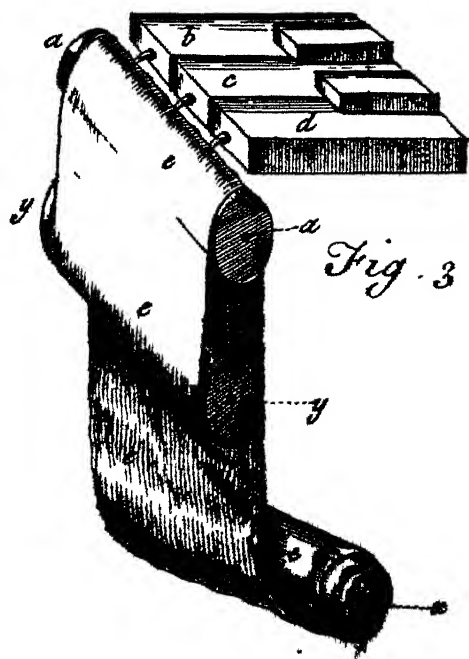
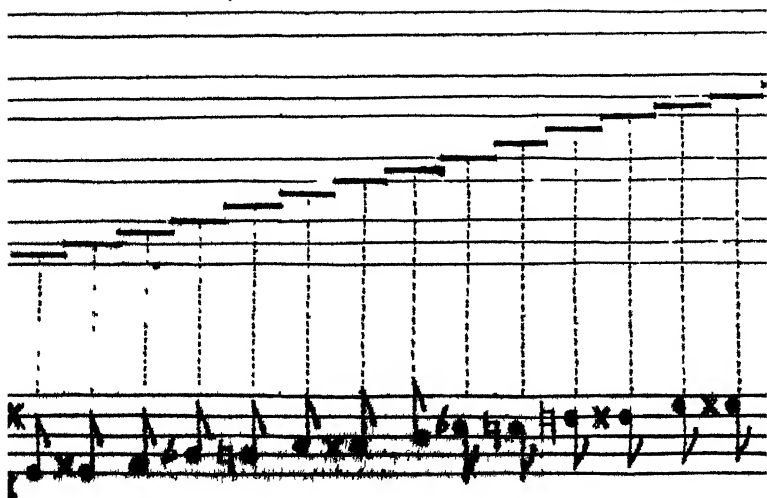


Fig. 3. p. 4.

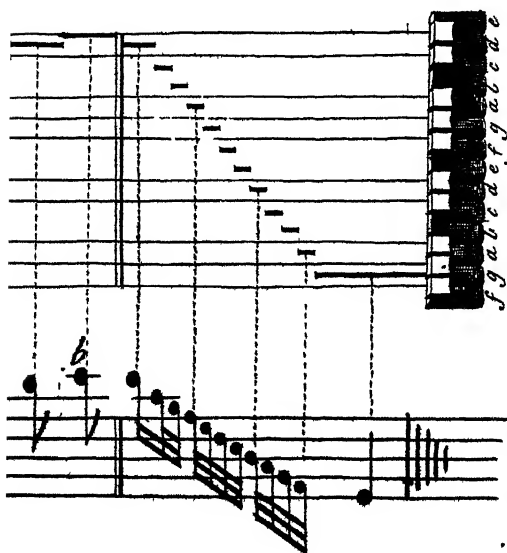
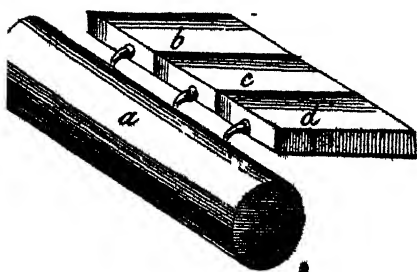


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I. *Of Birds of Passage, by Mr. Mark Catesby,
F. R. S.*

*Read at a Meeting
of the Royal Society,
March 5. 1746-7.*

THE Places whereto Birds of Passage retreat when they take their Leave of us, are first of all to be inquired after; and then it will be proper to examine by what Road, and in what Manner they convey themselves to such Places wheresoever situated on our Globe.

The Reports of their lying torpid in Caverns and hollow Trees, and of their resting in the same State at the Bottom of deep Waters, are so ill attested, and absurd in themselves, that the bare Mention of them is more than they deserve. Of much the like Stamp is a late broach'd Hypothesis, which sends them above our Atmosphere for a Passage to their Retreat; which to me seems as remote from Reason, as the Ethereal Region is from the Aëreal; through which last Region I cannot conceive any Obstruction to their Passage, when, by the Approach of our Winter, they find a Want of Food, and at the same time are directed, by Instinct, to resort to some other Parts of the Globe, where they may find a fresh Supply. For the Want of Food seems to be the chief if not the only Reason of their Migration. And tho' Titmice and other small Birds abide here the whole Winter, and subsist on Insects, which they find torpid, or in a State of Mutation, in the Crevices of the Barks of Trees, and other their Winter-Recesses, yet most Birds of Passage, having tender Bills, are

L I I

incapacitated

incapacitated for this Work; but then the Length of their Wings enables them to prey on numberless flying Insects, with which the Air is stored during the warm Months: And it is observable, that not only Swallows, but most other Summer Birds of Passage feed, on the Wing, on such-like Insects as are seen no more when cold Weather begins to come.

The various Conjectures concerning the Places whereto Birds of Passage retire, are occasioned by the Want of ocular Testimony to bring the Matter to some Certainty. But if the Immenseness of the Globe be considered, and the vast Tracts of Land which still remain unknown, unless to their own barbarous Inhabitants, it is no Wonder we are yet unacquainted with the Retreat of these itinerant Birds. If I may be allowed to offer my own Sentiments, I cannot but agree in the general Opinion of their passing to other Countries by the common natural Way of flying, with this additional Conjecture; *viz*, that the Places, to which they retire, lie probably in the same Latitude in the Southern Hemisphere as the Places from whence they depart; where the Seasons reverting, they may enjoy the like agreeable Temperature of Air.

It may be objected, that Places of the same Latitude in the Southern Hemisphere may be divided by too wide a Tract of Sea for them to pass over. But why then may not some other Parts of the Southern Hemisphere serve their Turn? This seems more reasonable to me, than that they should remain on our Side of the Northern Tropic; within a few Degrees of which, at the Winter Solstice, it is so cold, as frequently to produce Snow; which, by dispersing
such

such Insects as Birds, that feed upon the Wing, and particularly the Swallow Kinds subsist on, must make them perish inevitably, were they not to change their Quarters for those more favourable Climes, where a Continuance of warm Weather affords their natural and proper Food. This their Sagacity dictates to them, and is the apparent Cause of their periodical leaving us at the Approach of Winter, before Flies are so dissipated by Cold and Winds as to be found no longer in the Air; tho' they may with other Insects be met with in Holes and hidden Recesses, and serve to subsist other Birds of Passage.

What I infer from hence is, that as Swallows cannot continue and subsist so long in cold Seasons as other Birds of Passage, they are necessitated to visit us somewhat later, and to depart sooner: For tho' Nightingales, and other Birds of Passage, are not often seen or observed after they cease singing, yet I have frequently taken notice of them in their solitary Coverts a Month after the Departure of Swallows. From these Reasons I therefore conclude, that Birds of Passage, particularly Swallows, are necessitated to pass the Tropic of *Cancer*; but how far more South, or to what Part of the Southern Hemisphere they go, remains unknown.

The Manner of their journeying to their Southern Abode may vary, as the different Structure of their Bodies enables them to support themselves in the Air: Those Birds with short Wings, such as the Red-start, Blackcap, &c. tho' they are incapable of such long Flights, and with so much Celerity, yet I can't see why they may not pass in the like manner, but by gradual and slower Movements. Swallows

and Cuckows may probably perform their Flight in half the Time; yet there seems no Necessity for a precipitate Passage, because every Day's Passage affords them Increase of Warmth, and a Continuance of Food a longer Time than is necessary for their Passage, were it to the same Latitude South as that from whence they came.

As Providence in many Instances has guided defenceless Animals to make use of the most necessary Means for their Security, why may not these, and other itinerant Birds, perform their long Journeys in the Night-time, to conceal themselves from rapacious Birds, and other Dangers that Day-light exposes them to; which nocturnal Travelling of Birds of Passage I have Reason to believe more than barely probable, from the following Observation, which may serve in some degree to confirm it:

Lying on the Deck of a Sloop on the North Side of *Cuba*, I, and the Company with me, heard three Nights successively Flights of Rice-Birds (their Notes being plainly distinguishable from others) passing over our Heads northerly, which is their direct Way from *Cuba*, and the Southern Continent of *America*, from whence they go to *Carolina* annually at the time Rice begins to ripen; and, after growing fat with it, return South back again.

The Flight of Birds of Passage over the Seas has by some been consider'd as a Circumstance equally wonderful with other Stories concerning them; and especially in regard to those with short Wings, among which Quails seem, by their Structure, little adapted for long Flights; nor are they ever seen to continue on the Wing for any Length of Time; and

and yet their Ability for such Flights cannot be doubted, from the Testimony of many. *Bellonius* in particular reports, that he saw them in great Flights passing over and re-passing the *Mediterranean* Sea, at the Seasons and Times they visit and retire from us.

The same Sagacity that instructs them to change Climates may also reasonably be thought to direct them, and other Birds of Passage, to the narrowest Part of our Chanel, thereby to evade the Danger of passing a wide Sea; tho', by the many Instances I have seen of Birds driven Hundreds of Miles from any Land, there seems not that Necessity for their finding the Streights of *Calais*, as the shortest Passage to our Island, they being not unable to perform much longer Flights.

There are also Winter-Birds of Passage, which arrive here in Autumn at the time the Summer Birds depart, and go away in the Spring Season, when Summer Birds return. These however are but few; there being only four Sorts that I know of; *viz.* the Fieldfare, Redwing, Woodcock, and Snipe; which two last I have frequently known to continue the Summer here, and breed; so that the Fieldfare and Redwing seems to be the only Birds of Passage that constantly and unanimously leave us at the Approach of Summer, retiring to more Northern Parts of the Continent, where they breed, and remain the Summer, and at the Return of Winter are driven southerly from those frigid Climates in search of Food, which there the Ice and Snow deprives them of. There are many others, particularly of the Duck and wading Kind, that breed, and make their Summer

mer Abode in desolate fenny Parts of our Island. When the Severity of our Winters deprives them of their liquid Sustenance, Necessity obliges them to retire towards the Sea in numerous Flights, where in open brackish Waters they find Relief, and at the Approach of the Spring they retire to their Summer Recesses. But these cannot be included among those usually termed Birds of Passage.

Besides the different Kinds of Swallows, I know of but one Kind of *European* Bird that subsists in like manner by catching its Food on the Wing, and that is the *Caprimulgus*, or Goat-sucker, the capacious Structure of whose Mouth and Gullet is formed to receive Insects of the larger Kinds, as *Scarabæi*, *Grillo-talpæ*, &c. These are also Birds of Passage.

We have, 'tis hoped, made it pretty evident, that Summer Birds of Passage come to and depart from us at certain Seasons of the Year merely for the sake of a more agreeable Degree of Warmth, and a greater Plenty of Food; both which Advantages they procure by an alternate Change of Climate; but the Migration of Winter Birds of Passage, and particularly of Fieldfares and Redwings, is much more difficult to be accounted for, there being no such apparent Necessity, either on the Score of Food or Climate, for their Departure from us.

The Reason of their coming here in Winter is, 'tis highly probable, for the sake of Food, and a more suitable Climate than that they leave behind them; but, in some severe cold Seasons, and when there is a Scarcity of Berries they subsist here with Difficulty, and are even famished sometimes for Want of sufficient Food; yet what appears most unaccountable

ountable is, that such as have continued with us a whole Winter in Penury, and should, one would imagine, rejoice at our approaching Spring, and build their Nests, and couple, on the contrary all depart; as if that mild and pleasant Temperature, which delights and cherishes most other Creatures, were disagreeable to them. We know the Places of their Summer Retirement to be *Sweden*, and some other Countries in that Latitude; but, as they would find those Countries too cold for their Reception, and probably destitute of Provision, were they to hasten directly thither when they depart from hence, they journey gradually, and prolong their Passage through the more moderate Countries of *Germany* and *Poland*, by which means they don't arrive at those Northern Regions, adapted by Providence for their Summer Abode, and the Breeding of their Young, till the Severity of the Cold is so much abated as to render it pleasing to them, and Food may be there found. When they visit us again in Winter, their Return back is after the same Manner.

The Winter-Food of these Birds being Berries, and particularly Haws, as a greater Abundance of them grow in this Island than can be supposed in the more Northern Regions, that may possibly be one great Allurement to bring them over hither: But the principal Reason inducing them to travel Southward is probably the Rigour and Severity of the Cold in those frigid Climes, which Nature therefore directs them to desert for such as are more temperate.

A *Swedish* Gentleman informed me some Years ago, that, observing the Use we make of quick-set Hedges

Hedges in *England*, he sent some Plants of the white Thorn over to his own Country for the same Purposes; before which time he assured me there were none of them in *Sweden*, which I mention, in Support of what was said above.

The coming of these Birds to us may then pretty well be accounted for from the Reasons foregoing; but the Cause of their Departure from hence at the time they leave us, is one of those Secrets in Nature which are not yet discover'd; for, should it be suggested, that they do not leave us till the Haws and Berries are all gone, and they are under a Necessity to seek for Food elsewhere, this would amount to little, unless it could be shewn, that the Northern Regions to which they journey can afford them a fresh Supply; which it is almost certain they cannot. And therefore, when first they go from us, they must either alter their Diet, or be in much Distress; but, as 'tis evident their Food in the Summer-time must be of a different Kind than what they eat in Winter, 'tis most likely they change their Diet; and then one would imagine they should find Subsistence here in greater Plenty, and much sooner than in the colder Countries to which they fly.

In short all, we can be said to know of the Matter, ends in this Observation, that Providence has created a great Variety of Birds, and other Animals, with Constitutions and Inclinations adapted to their different Degrees of Heat and Cold in the several Climates of the World (whereby no Country is destitute of Inhabitants) and has given them Appetites for the Productions of such Countries, whose Temperature is suited to their Nature, as well as Knowledge

ledge and Abilities to seek and find them out: From which we may infer, that the Birds we have been mentioning could no more subsist in the sultry Climes of the *Antarctica* Isles, than Birds of Paradise could in the frigid Regions of *Sweden* or *Lapland*.

Besides the migratory Birds already mention'd, which breed and remain the whole Summer, there are other Birds that arrive periodically at certain Places for the sake of some Sort of Grain, or other Food, which may be supposed their own Country is destitute of: These Birds, after no long Continuance, depart, and are no more seen till that Time twelve Months after; at which time they return, and so continue repeating these annual Visits, as has been already observed of the Rice-Bird, and Blue-Wing of *Carolina* (*Nat. Hist. Florid. &c.* Vol. I. p. 14, & 99.) Tho' the secret Ways by which Instinct guides Birds, and other irrational Creatures, are little known to us, yet the Causes of some of their Actions are apparent.

Analogous to the lucrative Searches of Man thro' distant Regions, Birds take distant Flights in quest of Food, or what else is agreeable to their Nature; and when they discover some new Grain, or pleasing Food, they return and acquaint their Community therewith, and joining in numerous Flights, make annual Excursions to solace on this their exotic Food.

Since the Discovery of *America* there have been introduced from *Europe* several Sorts of Grain, which were never before known in that Part of the World, and which not before some Length of Time were found out, and coveted by some of these migratory

Birds. No Wonder this Grain should not be immediately known to Birds of distant Regions; for above half a Century passed from the Time of cultivating Wheat, Rice, and Barley, in *Virginia* and *Carolina*, before those Grains were found out and frequented by these foreign Birds, of which one has but lately made its first Appearance in *Virginia* as my ingenious Friend Dr. *Mitchel* informs me, that he being in his Garden a Bird flew over his Head which appeared with uncommon Lustre, and surpris'd him the more, not having seen the like Bird before. Mentioning this to some of his Neighbours, he was told by them, what afterwards was confirmed to him by his own Observation; *viz.* that these exotic Birds had but within these few Years appeared in *Virginia*, and had never been observed there before.

They arrive annually at the time that Wheat (the Fields of which they most frequent) is at a certain Degree of Maturity; and have constantly every Year from their first Appearance arriv'd about the same time in numerous Flights. They have attain'd the Name of *Wheat-Birds*.

II. *A Letter from Mr. John Freke F. R. S. Surgeon to St. Bartholomew's Hospital, to the President of the Royal Society, inclosing a Paper of the late Rev. Mr. Creed, concerning a Machine to write down Extempore Voluntaries, or other Pieces of Music.*

S I R,

*Read March 12.
1746-7.*

I THINK the inclosed Paper is the Effect of great Ingenuity and much Thought; and as the Subject-Matter of it may tend to give great Improvement and Pleasure to many, not only in our own Country, but every-where, I hope my presenting it may not be thought improper that it may thereby be printed and published to the World.

It was invented and written by Mr. *Creed*, a Clergyman, who was esteemed, by those who knew him, to be a Man well acquainted with all kinds of mathematical Knowledge. It was sent me by a Gentleman of very distinguished Merit and Worth; if therefore from hence this Paper shall be thought proper to be published in the *Philosophical Transactions*. It will prevent its being lost to Mankind. I am,

S I R,

Your very humble

and obliged Servant,

John Freke.

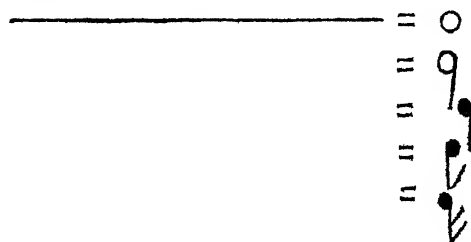
A Demonstration of the Possibility of making a Machine that shall write Extempore Voluntaries, or other Pieces of Music, as fast as any Master shall be able to play them upon an Organ, Harpsichord, &c. and that in a Character more natural and intelligible, and more expressive of all the Varieties those Instruments are capable of exhibiting, than the Character now in Use.

Maxim I.

ALL the Varieties those Instruments afford fall under these three Heads: *First*, The various Durations of Sounds, commonly called *Minims*, *Crotchets*, &c. *Secondly*, The various Durations of Silence, commonly called *Rests*. *Thirdly*, The various Degrees of Acuteness or Gravity in musical Sounds, as *A re*, *B mi*, &c.

Maxim II.

Strait Lines, whose Lengths are geometrically proportion'd to the various Durations of musical Sounds, will naturally and intelligibly represent those Durations. *Ex. gr.*



The first Line (being 2 Inches) represents a *Semibreve*.

The second is 1 Inch, and denotes a *Minim*.

The third is half an Inch, and signifies a *Crotchet*.

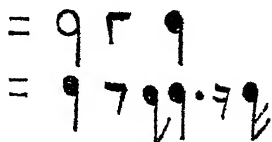
The fourth is a Quarter, and answers to a *Quaver*.

The fifth is an Eighth, and stands for a *Semi quaver*.

Max m

Maxim III.

The Quantity of the blank Intervals, or Discontinuity of the Lines, will exactly represent the Duration of Silence or Rests. *Ex. gr.*

*Maxim IV.*

The different Degrees of musical Sounds, as *Ga-mut, A re, B mi*, &c. may be represented by the different Situations of those black Lines upon the red ones or faint ones. *Ex. gr. see TAB. I. Fig. 1.*

Problem.

To make a Machine to write Music in the aforesaid Character as fast as it can be play'd upon the Organ or Harpsichord, to which the Machine is fixed.

Postulatum.

That a Cylinder may be made by the Application of a circulating, not a vibrating, *Pendulum*, to move equally upon its *Axis* the Quantity of 1 Inch in a Second of Time, which is about the Duration of a *Minim* in *Allegro's*;

Suppose the Cylinder *a* (see *Fig. 2.*) to be such, and to move under the Keys of an Organ, as *b, c, d,*

and nail Points under the Heads of the Keys, it is manifest, that if an Organist play a *Minim* upon *c*, that is, if he press down *c* for the Space of a Second, the Nail will make a Scratch upon the Cylinder of 1 Inch in Length, which is my Mark for a *Minim*.

Again, if he rest a *Crotchet*, that is, if he cease playing for the Space of half a Second, the Cylinder will have moved under the Nails half an Inch without any Scratch; but if the Organist next presseth down *d* for the Space of half a Second, the Nail under *d* will make a Scratch upon the Cylinder half an Inch long, which is my Mark for a *Crotchet*. It will likewise be differently situated from the Scratch that was made by *c*, and consequently distinguished from it as much as the Notes now in Use are from one another by their different Situation in the Lines. (*Vide Fig. 1.*)

These three Instances include all that can be performed upon an Organ, &c. (Maxim I.)

Therefore it is already demonstrated, that whatever is play'd upon the Organ during one Revolution of the Cylinder *a* (*Fig. 2.*) will be inscribed upon it in intelligible Characters. — I proceed to shew how this Operation may be continued for a long time.

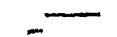
In *Fig. 3.* *aa, b, c, d,* are the same as in *Fig. 2.* Let *x* be a long Scroll of Paper wound upon such a Cylinder as *z*. Let *eeee* be the same Scroll brought over the Cylinder *aa*, to be wound upon the Cylinder *yy*, as fast as the Motion of *aa* (which is determined by a *Pendulum*) will permit.

It is manifest, that whatever is play'd upon the Organ during the winding up of *yy* will be written on the Scroll by the Pencils *b, c, d, &c.*

All

All the Graces in Music being only a swift Succession of Sounds of minute Duration will be expressed by the Pencils by small Hatches geometrically proportion'd to those Durations. *Ex. gr.*

A single Beat



A double Beat



A Shake

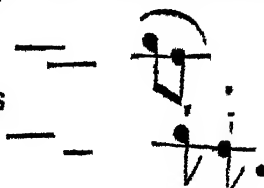
A Turn

A single Backfall

A double Backfall

A Shake and Turn

If a Line commence exactly over or under the Termination of another, it is an Indication of a *Slur*; as



So a small Interval indicates the contrary; as



Flat or sharp Notes are implied by their Situation on the red Lines; the natural Notes being always drawn between them, *viz.* in the Spaces. (*Vide* Fig. 1.)

The Scroll may be prepared before-hand with red Lines to fall under their respective Pencils. It is the surest Way to rule them after; tho' it is feasible or possible to contrive that they may be ruled the same Instant the Music is writing.

The Places of the Bars may be noted by two supernumerary Pencils, with a Communication to the Hand or Foot of a Person beating Time.

Grave

Grave Music from brisk, flow from fast, &c. will be better distinguished by this Machine, than in the ordinary Way by the Words *Adagio*, *Allegro*, *Grave*, *Presto*, &c. for, by these Words, we only know in general this must be slow or fast, but not to what Degree, that being left to the Imagination of the Performer; but here I know exactly how many Notes must be play'd in a Second of Time; *viz.* as many as are contain'd in 1 Inch of the Scroll *per Postulatum* P. 447.

Lattly, Whereas, in the ordinary Way of writing Music, you have either no Character for Graces, or such as do not denote the Time and Manner of their Performance, here you have the minutest Particles of Sound that compose the most transient Graces mathematically delineated.

N. B. Tho', to facilitate the Demonstration, I suppose the Pencils to be fixed under the Heads of the Keys, and consequently to require a very broad Scroll to pass under them; yet I intend the Pencils a more commodious Situation, *viz.* the Motion of the Keys to be communicated by small Rods to them (which I know better how to do than to describe, the Scheme would be so perplex'd). The Pencils are to be made of Steel, and ranged in close Order like the Teeth of a small Comb, so that a very narrow Scroll will do. I can prepare the Paper to receive a very black Impression from the Pencils at so cheap a Rate, that, at the Expence of 6 *d.* in Paper, I can take in Writing all the Musick that the swiftest Hand shall be able to play in an Hour.

III. *The Figure of the Mustela fossilis; communicated from Dr. Gronovius at Leyden to Mr. Peter Collinson, F. R. S.*

Read March 12. 1746-7. **M**USTELA fossilis, sive Cobites carulefscens, lineis quinque nigris longitudinalibus. Arted. Ichthyol. gen. xi. 3. *. Vide TAB. II. Fig. 1.

This Fish was kept alive in a Jar of Water a Year wanting 9 Days, without changing the Water, and without any other Food than what the Water afforded. They dig them out of the Sands near *Wesel* in *Holland*.

IV. *Some Observations on the Belluga-Stone, by Mr. Peter Collinson, F. R. S.*

Read March 12. 1746-7. **T**HESE Stones of the *Belluga* were collected by Dr. *Cook* at *Astracan*, and sent to Dr. *Sanchez* at *Petersburgh*, by whose Favour they came to me. I have applied to those Gentlemen to satisfy my Inquiries about them, and the Accounts they have communicated, with my own Observations, are as follows:

The *Calculus* of the *Belluga* is found of various Shapes and Sizes; it is mostly of a flatted oval Figure,

* Willoughby, *Hist. Pisc.* p. 124. TAB. G, 3, 4. Raii *Syn. Pisc.* p. 69.

gure, sometimes roundish, globular with unequal Depressions, and of a yellowish white Colour externally, and a smooth polished Surface.

It differs in Magnitude, as it does in Figure, from the Bulk of a Pigeon's Egg up to four or five times that Size.

They are mostly compact, ponderous and solid, not very friable, but requiring a pretty smart Blow of a Hammer to break them. They yield easily to the Saw; but this defaces their internal Texture, which is very remarkably elegant and regular. The Stones consist of concentric Coats firmly adhering to each other, formed about a *Nucleus*, which appeared to be quite an heterogeneous Substance, both from its Colour, Hardness, and Texture.

But another obvious Circumstance in its Structure renders the *Belluga* Stone different from most others, which is its radiated Appearance. It seems composed of an infinite Number of shining Rays, regularly diverging from the central *Nucleus* to the Circumference, representing both in Colour and Form the Flakes of a pure white *Terra foliata Tartari*, or (excepting the Colour, which is yellowish) the striated *Spicula* of Antimony.

This Stone is found in the Fish called the *Belluga*, a Species of Sturgeon, the *Acipenser tubercalis carens Artedii*, Part III. pag. 92. It is commonly called *Lapis Bellugæ*, by the *Russians* *Kamen Belluga*, which signifies the same thing.

Of this Fish several Authors have given us the following Account; in Shape it is not much unlike
a Sturgeon,

a Sturgeon (*a*); only its Snout is proportionably shorter and thicker; the Skin on the Back is light-grey, but under the Belly it is white, and without Scales (*b*): Its Flesh is whiter than Veal; whence the Name *Belluga*, or the white Fish; and affords a much more delicious Dish (*c*) than Sturgeon. Of its Row or Spawn is made the *Cavear*; and some are found so large as to yield from 156 to 200 Weight of it. They are found in greatest Plenty, and especially those of the largest Size (*d*) in the River *Volga*, about the City of *Astracan* (*e*). *Strahlenberg* says, he saw one caught in this River 56 Feet long and 18 Feet thick; and takes them to be the largest River Fish in the World. They are likewise found in other Rivers, as the *Don*, and those that flow into the *Baltick* and *Caspian* Seas.

I am not certainly informed, neither do Authors agree, in what Part of the Fish this Stone is found; *Strahlenberg* says, in the Head and Stomach; some (*f*) say, in the Air Bladder; others in a particular Bag near the *Anus* or inferior Gut; others again in still different Parts. It is found in both Sexes, but ofteneft in the Male, and of all Ages; but is very rare and scarce, for in a thousand Fish it often happens not to meet with a Stone.

From hence it would appear, that these Stones are preternatural to the Fish; perhaps morbid Productions, just as the Stone in the human Bladder, notwithstanding its curious and regular Form; probably

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(*a*) *Vide* Crull's *History of Russia*. (*b*) *Strahlenberg's History of Siberia*. (*c*) *Crull's History of Russia*. (*d*) *Strahlenberg ibid.* (*e*) *Crull's Hist. of Russia*. (*f*) *Dr. Cook's Letter*.

the Food of the Fish; the Situation of the Parts in which it is generated, and many other Circumstances, may contribute to this Uniformity of Appearance.

A little of this Stone scraped, and laid upon an hot Iron, gave a faint urinous Smell, and calcined into a light, greyish, insipid Earth.

Had it been a real animal Substance, or a constituent Part of the Animal, its Smell would, in all Probability, have at once discover'd it.

The Natives about the *Volga* very much esteem this Stone for its Virtues, being in great Reputation to promote Delivery. The common People take from 10 Grains to 30, 40, or even 60 scraped fine in a little Water, 2, 3, or 4 times in 24 Hours, when the Case is dangerous.

It is also highly commended as a Diuretic and Lithontriptic; and this not only amongst the common People, but amongst such as are more capable of informing themselves of its Effects.

References to the Figures in TAB. II.

Fig. 2. an oval Stone, flat and rugged on its under Side; Part of which *a, b.* has been scraped away, and is broken into two Pieces by the Crack *c, d.* at *e* and *f* appear 2 *Nuclei* or Centers of smaller Incrustations near the Surface of the larger Stone.

Fig. 3. Is the larger Fragment of the same Stone, or the Side *b, c, d.* at *g* is a Cavity answering to the Protuberance *b*, in the next Figure. *i, i, i,* are the shining Rays diverging from the central *Nucleus*.

Fig. 4. The smaller Fragment, or the Side *a, c, d.* in which the smaller *Nuclei e* and *f.* of fig. 2. appear. *b* is the central *Nucleus*, which fills up the Cavity *g* in fig. 3. and *i, i, i,* are the like Sprays or *Spicula* as in fig. 3.

Fig. 5. A smooth oval Stone, in Form of a long Egg.

Fig. 6 and 7. This Stone split in two Pieces, *a, b* and *c, d.* *e* in fig. 6. is the *Nucleus* or Center of the Stone, which seems to have been a Tooth of a Fish, and filled up the Cavity *f* in fig. 7. and *i, i, i, i,* are the Sprays or *Spicula* in both Figures.

V. *An Observation on an Occultation of Cor Leonis by the Moon, on Thursday, March 12. 1747, in Surrey-Street in the Strand, London, with a reflecting Telescope, made by Mr. Short, F. R. S. which magnified about 100 times; communicated to the Royal Society by J. Bevis, M. D.*

Read March 19.

1746-7.

Apparent Time.
 d h ' "

1747, Mar. 12	8 24 19	The Star immerg'd into the dark Limb.
-	9 27 4	It emerg'd from the enlighten'd Limb a small Matter to the West of the Moon's Zenith.
	44 4 $\frac{1}{2}$	The Moon's preceding Limb pass'd the Meridian in the Transitory.
	44 21	The Star passed the Meridian.

Mr. *Short*, another Gentleman, and myself, agreed to a single Second in the Immersion, with different Telescopes; but I saw and pronounced the Emer-
 sion 2 or 3 Seconds before them. — There had been an exact Observation of the Sun's Transit at Noon; and the Clock gain'd about half a Second a Day.

We


We reckon *Surrey-Street* 27 Seconds in Time West of the Royal Observatory at *Greenwich*.

Mr. *John Catlin* had a few Days before deliver'd me a Computation of this Eclipse, corrected from two Places of the Moon, observ'd the 28th of *February* and the 2d of *March* 1729, corresponding pretty nearly with her present Situation; as likewise from the Star's Position, as I had rectify'd it from several late Observations; and this gave the

Immersion at	— —	8 26 $\frac{1}{2}$
Emerfion	— —	9 30

J. Bevis.

VI. *An Observation of an uncommon Gleam of Light proceeding from the Sun, by Mr. Peter Collinson F. R. S.*

Read March 19. 1746-7.  N the 8th of *March* 1746-7, near 8 o' Clock in the Morning, as I was riding within three Miles of *Brentwood* in *Essex*, there appeared a singular *Phænomenon* in the Heavens; the Sketch (TAB. . Fig. .) may give some Idea of it.

The Morning was fine and clear, the Sun shone bright, no Cloud to be seen, but the Air a little hazy: Where the *Phænomenon* appeared, which was a bright cloudy Spot, seem'd a very small Portion of a Rainbow,

a Rainbow, only the Colours very faint. It was in a horizontal Direction North of the Sun, and from it projected a long luminous Ray, which terminated in a Point. — It continu'd very strong for more than half an Hour after I saw it, and then vanish'd away by degrees.

P. Collinson.

VII. *Extract of a Letter from Mr. Benjamin Cooke F.R.S. to Mr. Peter Collinson F.R.S. concerning the Property of new Flanel sparkling in the dark.*

Newport, Isle of Wight, Jan. 13. 1746-7.

Read March 19.
1746-7.

SINCE I have read the *Transaction* N^o. 476. with respect to the sparkling Lady, who could communicate a kind of electrical Fire to her Garments, I can give you an Instance nearly like it, of a Lady who was surpris'd at such an Appearance from a Flanel Petticoat, which she happen'd to shake in the dark. But at last we found, that new Flanel, after some time wearing, would acquire this Property; but that it lost it by being washed.

VIII. *A Letter from Mr. John Hill, Apothecary, to the President, concerning Windsor Loam.*

Read March 19.
1746-7.

AN Accident calling me Yesterday to *Hedgerley*, the Place where there is dug an Earth commonly call'd *Windsor Loam*, and famous not only in *England*, but many other Parts of the World, I took an Opportunity of going to the Pits, and informing myself of the present Condition of them : And as there appears too much Probability that this Earth will be exhausted, and lost intirely to the World, in a few Years, I presume it may not unacceptable to you to have an Account of the Pits of it, and whatever else relates to it, taken on the Spot ; which I here do myself the Honour of communicating to you, and shall take the Liberty of adding to it what has since occur'd to my Thoughts in regard to the supplying its Place when lost, in the many different Occasions on which it is now us'd.

This Earth itself is a coarse harsh Loam, composed of a very large shining Sand, of extreme Hardness, and a fine soft tenacious Clay : Its Value is its remarkable Quality of standing the Force of the most violent Fires without running to a Glass ; which makes it extremely useful to all who have Occasion for such Fires, and is the Reason of its being sent not only into all Parts of *England*, but to *Holland*, *Germany*, and many other Parts of the World. It is used for making the
Bricks

Bricks employed in building the Wind-Furnaces for melting Iron, for coating over the Insides of Assay-Furnaces, used by the Workers on Metals, and on many Occasions of like kind at the Glass-Houses, both in *England* and other Nations.

The Place where it is dug is *Hedgerley* before-mention'd, a small Village about 22 Miles from *London*, surrounded with Hills, under one of which this Loam lies. The Pits are about a Quarter of a Mile South-west from the Town, and five Miles North of *Windsor*: They extend over four Acres of Ground, situated on the Descent of a Hill; and were intended to have been carried over much more Ground by the Person who now works them; but, on Trials, the Loam is found not to extend as was imagin'd.

They dig, before they come at this, a very good common Brick-Clay, a Tile-Clay, and a Potter's Earth, a kind of Clay of a firmer Texture, and deeper Colour, than either of those; but the *Strata* of these are seldom pure or regular, and at the Boundaries of the *Stratum* of Loam a pure hard Sand evidently the same with that in the Composition of the Loam, but left loose, from there not having been Clay in the Way to bring it into the Condition of the perfect Mass. They have already work'd the *Stratum* so far as to find it bounded East and West by Beds of this Sand, and Northward by Chalk, and are therefore afraid it will be soon exhausted; at least, whatever they get hereafter must be procur'd with more Labour and Expence, as they have no where to search for it but higher up in the Hill; from whence it must be fetch'd at greater Depths, and much more

Expenſe; and this increaſing Difficulty of procuring it has been the Reaſon of its riſing in its Price to that it is now ſold at, which is five Shillings a Buſhel in *London*; but which is not to be wonder'd at, ſince on the Spot the Quantity that makes a thouſand Bricks, which uſ'd to coſt 1*s.* and 8*d.* now coſts 10*s.* the Digging, and will every Year coſt more and more, unleſs a new *Stratum* of it ſhould be diſcover'd ſomewhere thereabouts, which their many unſucceſſful Trials make them at preſent deſpair of.

It is to be obſerv'd, that this valuable Earth forms but a ſingle *Stratum*, and that does not riſe and dip with the Elevation and Deſcent of the Hill, as the *Strata* of the Earth, Stone, &c. in Hills uſually do, but ſeems to be even and flat at its Bottom; for the higher up the Hill they open their Pits, the deeper in proportion they find the *Stratum* of Loam lie.

It is worthy Obſervation, that this Hill appears from this not to have been form'd as the Hills and Mountains on the Earth in general have been by a Diſruption and Elevation of the *Strata* by Violence from within the Earth; for, in that Caſe, this *Stratum* of Loam muſt have been elevated with them, and would have been as near the Surface, or nearly ſo, in one Part of the Hill as in another, and need have been dug for no deeper from the Top than from any other Part; whereas, on the contrary, it appears to lie flat and level underneath the whole Maſs of Earth, which makes the Hill, and was, in all Probability, the Surface, on the firſt Settling of the terreſtrial and other Matter from among the Waters of the Deluge.

The Earth, which makes the Hill, seems to have been a prodigious Mass of Matter, roll'd along by the irresistible Force of that immense Body of Water, and afterwards lodg'd upon it.

That this might be the Case, the immense Force of that vast Quantity of Water, and the Ease with which heavy Bodies are moved in Water, may serve to make probable; and what the more favours the Conjecture is, that the Earth which makes the Hill is not disposed in such regular pure *Strata* as the Earths settled regularly from the Waters always are, but seems evidently a mixed Mass, made by the jumbling together of various Kinds of Clay, &c. which are, in some Parts of it found pure, tho' not in whole *Strata*; and in others irregularly blended in different Proportions one with another; which, as the principal Matters that compose it are of very different Colours, *viz.* a red and a white Clay, is the more apparent. And this is further confirm'd, by there being none of those common extraneous Nodules found lodged in it, which are so frequent in the *Strata* of Clay formed by Subsidence; such as the *Lodus Helmontii*, *Pyritæ*, &c. These have settled with, and lodged themselves almost everywhere among those *Strata*; but it is no Wonder there are none of them here, if this Hill has been formed, as I imagine; since, in the rolling it along, they must naturally have been left behind: And I promise myself, that the Frequency of these Bodies in almost all our little Clay-pits, and the intire Absence of them in the vast Quantities of Clay that have been dug here, will be esteemed, by all who have look'd deeply into these Studies, one great Argument of

the Truth of this System; which may also extend perhaps to many other Hills as well as this.

As the Workmen are now obliged to dig this Loam at 26 Feet deep, instead of about 14, at which Depth they long found it, and must hereafter, as they are obliged to ascend the Hill, dig it at 38 or 40 Feet, the Price of it will, I am afraid, rob us of it, before the Vein is exhausted. I think it would be a Matter worthy Consideration, whether, from examining the Parts it is composed of, a *Succedaneum* might not be found for it, by an artificial Mixture of similar Substances. In order to attempt this, I have, by means of Water, disunitd its Parts, and procured them separate; and, on comparing them with the various Earths and Sands from different Parts of *England*, which I have at times procur'd, I think that I can exactly match the Sand with one from *Hampstead-Heath*, and the Clay with one from a Pit near the lower End of *Highgate*: The Proportions may be easily learned, by accurate Observation of the Quantities of each, where disunitd; and a *Succedaneum* on these Principles easily made. I intend to attempt it, and recommend the Trial of it to Dr. *Mortimer*, who is well acquainted with the genuine, in *Chemical* and *Metallurgic* Operations, in both of which he is very conversant; and I hope to find it equally serviceable.

It is evident to me, that the only Reason why it endures the Fire so much better than other Clays, is the extreme Hardness and great Quantity of the Sand it contains: And as I imagine it easy to throw a Sand of equal Hardness, and in equal Quantity, into
an

an artificial Loam. I see no Reason to doubt of making it equally useful. I am,

S I R,

With all Respect,

Broadway Westminster,
May 28, 1746.

Your very obedient

humble Servant,

John Hill.

IX. *A Letter from Mr. Rob. Lucas, concerning the Relief he found in the Stone from the Use of Alicant Soap and Lime-Water, to his Brother the Rev. Mr. Richard Lucas F.R.S.*

Dear Brother,

Read March 26. 1747. I HAVE now the Satisfaction to acquaint you, that, by God's Blessing upon the Means His Providence directed me to the Use of, I am so far recovered of my Distemper (no other than the Stone), that I have not the least Doubt of being quite free from it in a little time. I am continually voiding Stones all broken, white on the Outside, without much Pain. I can now walk twice as fast as I could three Weeks since, without Uneasiness; nay I rode the other Day at once 24 measured Miles, trotting most of it, without Pain or Change of Urine; in one Word, I can be as certain of the dissolution

dissolving Power of my Medicines as I can be without seeing the Dissolution.

Dr. *Morgan* advised me to drink a Pint of Lime-water every Day. Colonel *Morgan* and this Lady coming on a Visit, advised me to take 4 Pills of *Allicant* Soap Morning and Evening; upon which I resolved to add the Soap-Pills to the Use of the Lime-Water; only, instead of the Quantity proposed, I took between 20 and 30 a Day, amounting to near an Ounce; which I thought I might safely do, well knowing, that Mrs. *Stephens's* Prescription amounted to almost 3 Ounces of Soap, besides other Ingredients: Afterwards I found in an Extract published in the Magazine, taken from Dr. *Whytt's* Treatise about dissolving the Stone in human Bodies, the Prescription of the very Medicine I used; only a Quart or three Pints of Lime-Water instead of a Pint, upon which I doubled my Quantity.

I have since borrow'd the Treatise myself, and would earnestly recommend the reading of it to every Person troubled with that Distemper. The Experience the Doctor has had of the real Effect of this Prescription in this Distemper, join'd with the many Experiments I have found of the dissolving Power of Lime-Water and Soap, gave me great Satisfaction.

I have used with great Success Stone-Lime newly calcin'd; but by those Experiments it should seem, that the dissolving Power of Lime-Water made of Oyster-Shells is almost double to that of Lime-Stone.

There are two good Qualities attending these Remedies; the first is, That they are cheap, easily come at, and prepared by one's self. 2dly, That they may
be

be safely used for a long time, without Danger to Health, I can vouch by my own Experience; for a Quart of Lime-Water, and an Ounce of Soap, has never given me the least Nauseating, Lowness of Spirits, or Abatement of Appetite, and I think I was never better in Health than I am now.

My Motive for being so particular in this Affair, is a Desire to be instrumental, by your means, of giving Ease to others in so unhappy a Condition; being firmly persuaded, that what has already so far reliev'd me, will, if prescrib'd, dissolve Stones of greater Magnitude than I suppose mine to be. From

Abergzeenny, Dec.
1746.

Your affectionate Brother,

Robert Lucas.

X. *The Figures of some very extraordinary calculous Concretions formed in the Kidney of a Woman; communicated by Mr. Charles Lucas at Dublin.*

See TAB. III.

Presented March 26.
1747.

PARTS of a *calculous Concretion* formed in the left *Kidney* of *Mary Anne Mac-Mahon*, otherwise *England*, taken out after her Death, in the 30th Year of her Age.

Figure

Figure I. A View of the anterior Part of the *Calculus* in its proper Situation, wanting, to complete its Form, *fig.* III. and v. and some other small Pieces which were joined or adhered to it at *A*.

Fig. II. A View of the posterior Part, completely the reverse of *fig.* I.

Fig. III. A View of another Portion, which, by the Intermediation of some smaller Pieces, was joined at *B* to *fig.* I. at *A*.

Fig. IV. The reverse of *fig.* III.

Fig. V. A Portion which seem'd broke off *fig.* I. at *A*; for it fitted it exactly at *C*.

Fig. VI. VII. VIII. Different Fragments, whose Places could not be certainly determined.

Fig. IX. A *Nucleus* of a dark Olive-Colour, and oval Figure, of the common Texture and Consistence of ordinary *Calculi*, discovered by cutting *fig.* v. transversely at *D*.

Fig. X. A transverse Section of *fig.* II. at *E*, very solid, white, and semi-pellucid, except at *F*, where a brown Vein, of the Colour of the Surface of the *Nucleus*, *fig.* IX. at *G*, and very porous, runs through it.

XI. *Part of a Letter from Mr. William Arderon F. R. S. to Mr. Henry Baker F. R. S. concerning the Formation of Pebbles.*

Dear Sir,

Read April 2. 1747.

IN my late Searches after Sands, Pebbles, and other Fossils, in our County of *Norfolk* (some whereof I had the Pleasure to send you not long ago) I made such occasional Observations on the Situation and Condition of the several Bodies I met with, as Reason must, I think, suggest to every Man that considers them. I shall trouble you with no Hypothesis, nor form any random GuesSES, to account for such their Situation, and the Condition wherein they are found; but, if a Relation of true Facts, and Conclusions naturally deducible therefrom, may prove acceptable, they are intirely at your Service.

In all *Strata* of Pebbles, that I have yet examined, there are some which are broken, and whose Pieces lie together, or very near each other; but, as Bodies of such Hardness could not be broken without some considerable Force or Violence, their Situation implies, that they suffered such Force or Violence as broke their Parts asunder, in or near the Place where they at present lie.

Others again have had Pieces broken from them, though not the least Fragment of those Pieces can now be found: From whence we must conclude, that whatever might be the Cause of their Fracture, they must either have been broken at some Place

distant from where they now lie, or the Pieces broken from them must at some time or other have been removed to some distant Place.

Several of these Pieces of broken Pebbles have their Edges and Corners so very sharp, that it seems as if they had never been removed from the Place where they received the Damage. Others have their Sides and Corners so blunted, rounded, and worn away, that one cannot help imagining they must have been very roughly tossed backwards and forwards against other hard Bodies, and that too with great Violence, or for a very long Continuance; since, without a great deal of Friction, such hard Bodies could scarcely have been reduced to the Forms they are now found in.

It may possibly be objected, that these Pieces of Stones grew in the Figure wherein they now appear; but I am fully satisfied, that any Man who will take the Pains to examine these Bodies carefully, will soon be convinced, from their Veins, or Grain, or Coats, which surround each other, somewhat like the different Years Growth in Trees, that they must once have been complete and intire: And this will be more fully evident, if they are compared with a Stone broken by Art.

Among these *Strata* of Pebbles are several Fragments of various Kinds of Marble, various Kinds of Sand-Stone, and various Kinds of *Gypsum* (though this Part of the Kingdom affordeth no such thing); most of which have attained the Hardness of the very hardest of our Pebbles, as it should seem, by lying amongst them.

- Such

Such Pebbles as are found here in *Strata* near the Surface of the Earth, are much more brittle, and break easier without Comparison, than those which lie in deeper *Strata*: For, if the first of these fall, but with their own Weight, upon any other Stone, from the Height of 3 or 4 Feet, they will break very frequently into ten or a dozen Pieces; whereas such as are found deep in the Earth will endure being thrown against one another with all the Force one can give, and that too twenty times perhaps, before the least Splinter of them can be broken off.

I have constantly found, that the more clean and transparent the Sands are with which our Pebbles are mix'd, the more beautiful the Pebbles themselves are, however different their Colours be.

It is wonderful to observe and consider with what amazing Skill the Creator of all Things hath disposed the different *Strata* of the Earth, to serve the Purposes of His Wisdom.

The vegetable Mould or Surface of the Earth is compounded or made up of Sands, Clays, Marls, Loams, rotten Stalks, and Leaves of Herbs, &c. serving as a proper Bed and Covering, as well as a Receptacle and Conductor of Moisture, to the Roots of Trees and Plants in general.

Sands and Pebbles may be consider'd as Drains for carrying off the redundant Moisture, to where it may be ready to supply the Place of what is continually rising in Exhalations; but, lest the *Strata* of Sand should be too thick, small ones of Clay are often placed between, and seem intended to prevent this Moisture from departing too far from where

it may prove of general Use. And, lest these curious but thin Partitions of Clay should give Way, by their Softness, for the Particles of Sand to insinuate into them, and thereby let the Moisture pass through, thin Crusts of a ferrugineous Substance are placed above and beneath each of these clayey *Strata*, and serve effectually to keep the Clay and Sand asunder.

The Observations you have now read must be understood to relate to the County of *Norfolk* only; for I have never had any Opportunity of searching the Bowels of the Earth in other Places; but the general Uniformity of Nature makes me suppose the Situation and Circumstances of Pebbles, Sands, &c. in other Countries may not be very different. Believe me,

S I R,

Norwich, March 3.

1745-6.

Yours, &c.

W. Arderon.

XII. *A Letter from Arthur Dobbs Esq; of Castle-Dobbs in Ireland, to the Rev. Mr. Charles Wetſin, Chaplain and Secretary to His Royal Highneſs the Prince of Wales, concerning the Diſtances between Aſia and America.*

S I R,

Recd April 9. 1747. **I** AM extremely obliged to you for the Trouble you have taken, in correſponding with Profeſſor *Euler* * upon the *Ruſſian*. Discoveries Eaſtward from *Kamſchatka*, and communicating to me the Accounts he had of *Behring's* laſt Voyage, and of his Discovery of the Lands North-Eaſt of *Japan*; which the Profeſſor could only have inaccurately, not having ſeen any Journal to fix the Latitude and Longitude of the Countries he then diſcover'd: But ſince Profeſſor *Euler*, ſway'd by the Opinion of Captain *Behring*, ſeems ſtill to believe that the laſt Land he diſcover'd is joined to *California*, which Country is now known to be Part of the Continent of *America*, and not an Iſland (in which Fact of its being continuous to *California* I differ ſtill in Opinion from him,) for, if that were a Fact to be depended upon, I would candidly own, that there could be no Paſſage from the North-Weſt of *Hudſon's-Bay* to the Western Ocean of *America*, without ſailing near 70° of Longitude; the Diſtance of the North-Eaſt Cape of *Aſia* from the North-Weſt of *Hudſon's-Bay*, in a Parallel almoſt as far North as the Polar Circle, before the Paſſage
can

* See *Transf.* N^o. 482, p. 421.

can be made to the *Pacific Ocean*; which might therefore be very reasonably call'd an impracticable Passage, as it could not possibly be made in one Summer, if at all); and since Professor *Euler* has been so kind as to give me Captain *Behring's* Reasons for supporting his Opinion, which are principally from the small Distance he supposed it was, from the Coast he discover'd, to the Western *American* Coast at *California* (which he imagin'd was much nearer his North-East Cape of *Asia* than it is in Fact); I must therefore, in return to the Professor's Goodness, in communicating to me all he has known in that Discovery, beg Leave to give you this further Trouble of communicating to the Professor my Reason for still dissenting from *Behring's* Opinion, that the Land he discover'd last was Part of the Continent of *America*, or continuous with *California*; and if he find the Reasons for supporting my Opinion make it more probable, that there still may be a large Opening betwixt these new-discover'd Countries and *California*, I am sensible it will give the ingenious and learned Professor great Pleasure, to think we may yet hope for a Passage by *Hudson's-Bay* to the Western *American* Ocean, without being obstructed with Ice after passing *Hudson's-Sreight*.

The Professor imagines I might have been led astray, by not considering, that the North-East Cape of *Asia* is much more Easterly than has been laid down in any former Charts; which is now known accurately, by the Eclipse of the Moon observed by Captain *Behring* at *Kamschatka*.

I have an Abstract of his Journal by me, upon his first Discovery in 1728, and 1729, when he observed that Eclipse, and the Calculation of the Longitude

from it ; and stand by his Longitude he has fix'd ; and allow that his North-East Cape is in the other Hemisphere ; reckoning Eastward, either from *Fero*, as the first Meridian, or from *London* ; which last I shall follow.

Behring fixes his North-East Cape $126^{\circ} 7'$ East Longitude from *Tobolski* ; and *Tobolski* is 86° East from *Fero* ; so the Cape is $212^{\circ} 7'$ East of *Fero*, or about 194° East from *London*. — By Captain *Middleton*'s Observation of *Jupiter's Satellite* at *Churchill* River in *Hudson's Bay*, that River is 95° West from *London* ; which, added to 194° , makes 289° ; consequently the North-East Cape of *Asia* is 71° distant from *Churchill*, to complete 360° ; which, in the Latitude of 65° , computing 8 Leagues to a Degree of Longitude, of which 20 make a Degree of Latitude, the Distance betwixt that Cape and *Hudson's Bay* would be 568 such Leagues.

From the known Longitude of the North Cape of *Japon* in 40° Latitude, which is pretty exactly known, from the Observations made by the Jesuits at *Peking*, and is about 150° East from *London*, and from the best computed Longitude of *California* in 40° North Latitude, it lies in 130° Longitude West from *London*, making together 280° , leaves 80° for the Distance of *California* from *Japon* ; allowing 17 Leagues to a Degree of Longitude in 40° North Latitude, the Distance would be about 1360 Leagues : By the same Calculation *California* must be at least 7 or 800 such Leagues from the North-East Cape of *Asia* ; so that, in so great a Space there may be very great Countries or Islands *, without supposing the new discover'd Country continuous to *California*, and might well allow of an open Chanel or Sea, from 50 to 100 Leagues wide, between the discover'd Coast and *California*.

By

By the Account given to Professor *Euler*, *Belring* sailed Southwardly to the Isles of *Japon*, and from thence sailed Eastwardly 50 *German* Miles, about 250 *English* Miles; which makes about 80 Leagues, of 20 to a Degree. At that Distance from *Japon* he discover'd Land, which he coasted North-West; still approaching towards the North-East Cape, without going ashore, until he came to the Entrance of a great River; where sending his Boats and Men ashore, they never return'd, being either lost, kill'd, or detain'd by the Natives, which made his Discovery incomplete; his Ship being stranded, and he afterwards died in an uninhabited Island.

As no Latitudes nor Longitudes are fixed by this Account, I must believe he sailed from *Kamschatka* South-East, perhaps more Southerly than to 50° Latitude; and there found Land North-East from *Japon*; otherwise, by coasting it North-West, he could never approach the North-East Cape, which is, at least, 40° Longitude East of *Japon*; and if he made Land 80 Leagues East of *Japon*, he must have sailed North-East to make the North-East Cape. I have therefore Reason to believe this Coast was Part of that he saw in his first Voyage, where he lost his Anchor; and is the Coast *Gama* discover'd, and the *Dutch* afterwards called the *Company's Land*, East of the Streights of *Uzicez*, which is at least 7 or 800 Leagues

* The *Japoneze*, in their Maps of the World printed in *Japon*, have laid down in this very Tract two Islands as large as *Ireland*, with the Names to them, as appears in that Map bought by Dr. *Kempfer* in *Japon* in 1686; now in Sir *Hans Sloane's* Museum.

Leagues West of any known Land of *America*, and above 1000 near the Latitude of *Japon*: So that, if I should allow 700 Leagues for Countries or Islands East of his new-discover'd Coast, there might still be a Passage of 100 Leagues for the Southern or *Pacific Ocean* to communicate with *Hudson's Bay*, and to cause such great Tides and Currents, as are found on the North-West of *Hudson's-Bay*; as also a free Passage for the Whales, which are seen in all the Openings North-West of that Bay, and are caught there in Numbers by the *Eskemaux* Savages: For, as these don't go in by *Hudson's Streight* from our *Atlantic Ocean*, it cannot be presumed that they should go up by *Japon* towards the North East Cape, and from thence go 70°, or above 560 Leagues, to *Hudson's Bay*, and be there in the Month of *June*, and, after staying until *September*, return again the same Way to the Southern Ocean, to pass the Winter.— Now, as *Bebring* only coasted at a Distance, he could not possibly know whether it was a Continent, or great Island; the last of which seems the most probable: However, a few Months now, if our Ships return safe, will give us a Certainty on one Side or the other; altho' I am sanguine enough to believe they have by this time sailed through, and discover'd this so much wish'd for Passage.

These, Sir, are the Reasons I have still to expect Success in the Attempt I have promoted; and, if you think it may give any Satisfaction to Professor *Euler* to know the Reasons that support my Belief of a practicable safe Passage, be pleas'd to communicate it to him, with my Compliments for the Trouble I

have given him by you, and accept of my best Acknowledgments for your Favours. I am, with the greatest Regard and Esteem,

S I R,

*Castle-Dobbs, Feb. 10,
1746-7.*

Your most obedient

humble Servant,

Arthur Dobbs.

XIII. *A Letter from the Rev. Mr. G. Costard, to the Rev. Thomas Shaw, D. D. F. R. S. and Principal of St. Edmund-Hall, concerning the Chinese Chronology and Astronomy.*

Reverend Sir,

Read April 30. 1747. **T**HE Subject of our late Conversation turn'd upon the Affectation of some Nations, in carrying up their Histories to so immoderate a Height, as plainly to shew those Accounts to be fictitious and without Foundation. This, it was agreed, was the Case of the *Babylonian* and *Egyptian* Accounts; and you seem'd to think it would be found to be the same with any other People that should make the like Pretensions.

The only People in later Times that have been thought to contradict this Opinion are the *Chinese*, of whose History the World hath been taught

taught to entertain very extraordinary Conceptions. But that even They will be no Exception to your Surmize, but, on the contrary, a strong Confirmation of it, will, I persuade myself, appear, from what I am now going to offer.

I need not inform you, that the Eastern Writers in general are much addicted to Fable and Romance. This is a Fact too well known to need any Proof; and therefore great Judgment is many times required to distinguish what is real from what is purely imaginary, improbable, and absurd. I say this, not so much with regard to their Accounts of foreign Nations, with whose Affairs they may be presumed to be less acquainted, as of their own ancient State and Condition, and that in Ages not exceedingly remote. But if this Observation holds but too true, with respect to those whose History we are in some manner acquainted with, how much ought it to put us upon our Guard as to those we are in great measure absolute Strangers to?

The best Accounts we have received of *China* are owing to the *Jesuits*. But those Accounts themselves are, I am afraid, to be frequently received with great Caution. These Fathers have been sometimes, perhaps, not sufficiently versed in *European* or *Chinese* Learning, or both, to give us proper Information. At other times, it may be, they have been too much prejudiced in Favour of their Converts, or had Ends to serve, of which the World hath not been properly enough apprised. To have propagated their Religion only in a barbarous and uncultivated Nation, would not have been so much for the Credit of the Mission, as to have been able to introduce it

among a People civilized and polished by Arts and Literature.

Suspicious as these and the like Circumstances are, is it not surprising to hear Authors, upon their Words only, and upon little or no Foundation besides (as I question not will appear), asserting with so much Positiveness, that the *Chinese History reaches up indisputably to the Times of Noah (a)*? A thing so far from being *indisputably* true, that no Article whatever perhaps will admit of greater Debate. 'Tis true indeed, the *Chinese* give us a long List of Kings that reigned among them from the Time of *Fo-hi*, and a Series of Dates, that, if allowed, may carry up his Age 2952 or 2847 Years before the Christian *Era (b)*. But how easy is it to feign * Dates and Successions of Kings! Let it be made appear what Foundation this Chronology depends on; what ancient Monuments the *Chinese* have, and in what manner preserv'd. Marbles, I suppose, they have none; and their Paper, such at least as is brought into *Europe*, appears to be of too fine a Consistence for the Preservation of Records.

You will be told, Sir, perhaps, that a great Part, at least, of their *Chronology* is verified by *Eclipses*. A very pompous Argument! but, when narrowly examined into, will be found to prove just nothing at all. We are told indeed (c), that the ancient *Chinese* Observations consist of 26 *Eclipses* of the *Sun*, and 21 Conjunctions of *Jupiter* with the fix'd Stars.

The

(a) *Shuckford's Connect.* Vol. I. p. 101.

(b) *Ibid.* p. 29.

* See these *Transf.* No. 415. p. 397, where this Chronology seems to be set in a true Light by the Viceroy himself of *Canton* 1724. *C.M.*

(c) *Obs. Math. Astron. Geogr. Chronol.* Tom. I. *Pref.* p. 13, 14.

The oldest *Eclipse* of the Sun is placed in the first Year of the Reign of *Tching-Cham* (d), supposed to coincide with the Year before *Christ* 2155: But the oldest Conjunction of *Jupiter* reaches no higher than the Year after *Christ* 73 (d): And how inaccurate the Observation was, appears from hence, that the *Chinese* only mark the Day when that Conjunction happened.

But the Question naturally arising here is, How it comes to pass, that the *Chinese* Accounts afford no Example of any planetary Conjunction before this, when they produce an *Eclipse* of the Sun 2228 Years earlier? By what good Fortune came that *Eclipse* to be preserved, and all Appulses of the *Planets* to fixed Stars for so many Years be lost? Let us suppose, that these were Things below the Notice of *Chinese* Astronomers; or that they did not know what Use to make of them. But in what manner must we account for this, That we hear nothing of any other *Eclipse*, till the Year before *Christ* 776 (f)? That all the *Eclipses*, observed during so long an Interval as 1379 Years, should have perish'd, and this *one* have escaped, requires a pretty strong Faith to believe.

But farther, we are told, that they observed the *Winter Solstice* in the Year before *Christ* 1111. There is nothing, it is true, impossible in this; for it is not said how accurate the Observation was. The
Difficulty

(d) *Ibid.* p. 18.

(e) *Ibid.* p. 15.

(f) Not much before the oldest *Babylonian Eclipse* that is preserved. See *Letter to M. Folkes Esq;* p. 21.

Difficulty is only to ascertain the Fact, and convince reasonable People that it was made at all.

'Tis well known, and allow'd by the Missionaries themselves, that the Reception they have met with in *China* hath been more owing to their Character as *Philosophers* than *Apostles* (g). When therefore they brought with them into the Country Accounts of *European* Discoveries, and particularly in *Astronomy*, might not the *Chinese*, agreeable to their vain-glorious Character, tell them, that they had of their own much older than any thing they could pretend to? It may be said indeed, that this is no more than Supposition, and which consequently argues but little: But then the Supposition is so easy and natural, that it requires at least the contrary to be made out by some very good Proof.

One Reason why this may be insisted on the more is, that the *Chinese*, according to the *Fathers* themselves, have not always been faithful in their relating Observations. *T-hang*, about the Year after *Christ* 721, had the Reputation among them of an able *Astronomer*; but being mistaken, it seems, in his Calculation of an *Eclipse*, rather than own his Ignorance, he pretended, that the *heavenly* Bodies did not always observe the same Laws. In Support of which extraordinary Hypothesis, he urged, that, in the Time of *Tsin* (h), the Star *Sirius* was eclipsed by the Planet *Venus*; tho' the Latitude of *Sirius* is $39^{\circ} 32' 8''$, and that of *Venus* never exceeds 4° . The same

(g) *Observat. ut sup. Tom. 2. p. 117.* (h) *Observat. ut sup. Tom. 2. p. 86. Flamsteed's British Catal. Greg. Astron. p. 5.*

same Sort of Observation with this, I suppose, is the other of the Conjunction of *Saturn, Jupiter, Mars, Venus, and Mercury*, in the Constellation *Cke*; when the *Sun* and *Moon* likewise were in Conjunction in 15° of *Aquarius*, in the Time of *Tchouen yu* (i).

And to put it out of all Doubt, that the *Chinese* are capable of obtruding upon the World *fictitious* Observations, we need no other Authority still than that of the *Learned Fathers* themselves. In the Year 1725 (k) the *Missionaries* sent into *Europe* an Account of an Approximation of the four Planets *Jupiter, Mars, Venus, and Mercury*. Such planetary Conjunctions, it seems, in *China*, are look'd on as happy Omens of good Fortune to the Prince upon the Throne. The *Chinese* therefore, as if bred up at the Court of *Versailles*, with a true *French* Politeness, in Compliment to their Sovereign, mark'd in their Registers a Conjunction of all the 7. This false Account of an imaginary Conjunction, as the *Learned Jesuit* himself observes, may, in future Times, be the Occasion of very great Errors. —To the *Chinese*, I suppose he means; for in *Europe* the Danger will be but small; where there are better Tables, exacter Accounts, and more accurate Observers, than the most sanguine *Jesuit* will pretend to be among the *Chinese*. But if they would venture at recording such a spurious Observation, at a time when they were sure of being detected, what may we not suspect them
to

(i) *Observat. ut sup.* Tom. 2. p. 149.
sup. Tom. 2. p. 33.

(k) *Observat. ut*

to have been guilty of, when they had none to confront them ; and how little may we presume they know of the Uses to be made of *Celestial* Observations?

We are told, with great Pomp and Assurance (1), That there *always* was in *China* an Office of *Mathematics*, and another of History : That it is the Business of the former to calculate *Eclipses* ; and of the latter to register them, and every other Occurrence that happens in the State.

It would have been well, if the *learned Fathers* had told us with a little more Exactness what we are to understand by the Term *always* ; and whether the *Chinese* are acquainted enough with the Uses for which *Eclipses* serve, to make it probable, that they should have had such an Office any *considerable Number of Years*, and much less *always*. May we not in the mean time suspect, upon hearing such Language as this, that the *Fathers* mistake *Pekin* for *Paris* ; and, having their Heads full of the *Academy of Sciences*, cannot help figuring to themselves the like in the remotest Corners of *Asia* ?

Be this as it will, they tell us, That the *Mathematicians* have often had the Credit to take out of the Registers their *false* Calculations, and substitute in their room others, agreeing with Observations. — But where Things are kept with so little Exactness, what can be expected but Confusion ? What less indeed can be expected than what the *Fathers* assure us themselves hath frequently been the Case (m), That,
by

(1) *Observat. ut sup.* Tom. 2. p. 158.
p. 159.

(m) *Ut sup.*

by this means *Eclipses* have been preserved, that Calculations made by *European* Tables demonstrate to be false? Can we wonder likewise, that the *learned Fathers* should doubt many times, whether such or such a particular Eclipse be an *Observation* made at the Time, or the Result only of a *Calculation*, and perhaps a false one (*n*)?

For, after all that hath been said of *Chinese Eclipses*, and the *Calculations* of them, it is agreed, (*o*) that, before the Time of *Lieou hong*, or *A. D.* 206. they had no fix'd Principles upon which to proceed in that Business. — This Observation, I am afraid, will extend to much later Times; or 'tis scarce to be imagined they should look upon total *Eclipses* of the Sun as ill Omens. — In consequence of this superstitious Belief, we are told (*p*), That the *Chinese Astronomers* have carried their Compliment to the Family on the Throne so far, as to affirm no such *Eclipse* could happen during their Time. On the other hand, should an *Eclipse* of this sort happen, without being foretold, they immediately pretend it to be a Warning from Heaven of some Misfortune likely soon to befall the Government. But if it should be foretold, and not come to pass, they would then make the many Virtues of the Sovereign the preventing Cause; and, what is better still, a Shelter for their own Ignorance. Such Notions as these however, I think, plainly demonstrate them to be very *bungling Astronomers*; and that they can hardly look upon these *Phænomena*

(*n*) *Obs. ut sup.* Tom. 2. p. 159.
Tom. 2. p. 32. (*p*) *Ibid.* p. 33.

(*o*) *Observat. ut sup.*

as depending on establish'd invariable Laws of Nature; the Consequence of which is, that they can no more attempt bringing them to a *Calculus*, than Winds, Thunder, and Lightning, and the like.

It was observed before, that the *Mathematicians* had many times the Art or the Credit to take out of the Registers their false Calculations; but we are told in the same Place, That, before they were re-positied there, they were presented to the Emperor, for his Inspection. Let any one, that knows the least of the despotic Governments of the East, reflect on the Probability of this; and whether the Attempter would not run great Danger of paying for it with his Head.

What hath been already said, will, I suppose, be more than sufficient to shake the Credit of *Chinese* Observations. But what must we think of those very ancient ones, when we are farther told (q), That, from the Time of *Tchun-tsieou*, or 480 Years before *Christ*, the *Chinese* themselves allow *Astronomy* was almost intirely neglected among them; and that *Tsin-chi-koang*, whose Reign began in the Year before *Christ* 246, order'd all Books of *History* and *Astronomy* to be burned (r)? But every one will easily imagine what Destruction of Observations must have been made during a Neglect of these Studies for 234 Years; and how little would remain to be burned by this *anti-astronomical Prince*.

It

(q) *Observat. ut sup.* Tom. 2. p. 1.

(r) *Ibid.* p. 2.

It was owing to this Devastation. it seems, that the *Chinese* are said to have lost the Method taught by the Ancients, and particularly the Emperor *Tao*, of calculating the Places of the 7 *Planets*, and the fixed Stars (*j*). It may with good Reason be questioned, whether they really had any Methods of calculating their Places at all: For to what Purpose could such Calculations serve, when their Catalogues of Stars, many Centuries later, are acknowledged to have contained nothing more than bare *Names*, without *Latitudes*, *Longitudes*, *Right Ascensions*, or *Declinations*? Such their Catalogues were, if they deserve that Name, that were made under the Race of Emperors called *Song* (*t*), or between *A.D.* 591 and 620; and it will be difficult to prove they were any thing else, before the *Jesuits* introduced there *Tycho-Brake's*, or other *European* ones. As to the Places of the Planets, how little they were able to compute them will appear from hence, that the utmost that *Lieou-hin* and *Lo-hia-hong*, in the Year before *Christ* 66, pretended to, was to calculate a plain rectangled Triangle (*u*). In what manner they did this is not said; but it would be well, if the *learned Fathers* would make it appear that the *Chinese* had, in much later Times, any thing like a Table of natural Sines and Tangents. A small Skill in *Mathematics* is requisite to apprehend from hence how bungling their Astronomy must have been; and if so, much more that of the Ages preceding them!

And

(*j*) *Observat. &c.* Tom. p. 3. (*t*) *Observat. &c.* Tom.
 p. 65. (*u*) *Observ. et sup.* Tom. 2. p. 8.

And it will be yet further hard to imagine that they knew how to find the Places of the Planets, when we are assured (x), that *Tchang-tse-tsin*, about *A.D.* 550, was the first Person that introduced Equations into their Computations of the Planets Motions; that *Co-chiou-king*, about *A.D.* 1280, was the first *Chinese* that knew any thing of *Spherical Trigonometry* (y); and that, before the Arrival of the *Jesuits*, they were intire Strangers to the Inclinations of the Planes of the planetary Orbits (z).

After what hath been said, I think we need but little more to convince us of the small Acquaintance of the *Chinese* with *Astronomy*. They tell us however themselves (a), That, from the Time of *Tsin-chi-hoang* above-mention'd, they had no expert *Astronomer*, no Books of *Astronomy*, nor known Method of computing. All that remained were some *confused Traditions*, *Catalogues of Stars and Constellations*, and *Fragments of Books*. A mighty Encouragement all this, to expect reforming the *European Astronomy* or *Chronology* by the *Chinese*! What sort of *Catalogues* these were, hath been already observed; and we may, without Offence, I presume, beg to be excused from paying over much Deference to *Chinese Tradition*; at least, till the Fathers have better determined what Degree of Assent it deserves.

About *A.D.* 164. several *Jew Families*, and other Subjects of the Western Empire, came into *China* (b). At that time, as it is allowed by the *Jesuits*

(x) *Observat. &c.* Tom. 2. p. 58, 59. (y) *Ibid.* p. 114.
 (z) *Ibid.* p. 84. (a) *Observ. ut sup.* Tom. 2. p. 3.
 (b) *Observ. ut sup.* Tom. 2. p. 119.

Jesuits themselves, *Ptolemy's Astronomy* was in great Vogue all over the East; and they seem to suspect, that by this means the *Chinese* might get some faint Knowledge of it. It is certain, that from this time we meet with Things unknown to their former Accounts of *Astronomy*. At this time, it is said (c), *Tchang-heng* made a Catalogue of 2500 Stars. Such a Catalogue as those already mention'd were, we may suppose it to be, if it was at all; for *Tchan-g's* Book is lost; and what Accounts we have of him or his Works, depend on the Authority of others.

A. D. 284. (d) liv'd *Kiang-ki*, the first *Chinese* that is said to have known any thing of the Motion of the fix'd Stars. This we see was 120 Years after the above-mention'd Arrival of the *Jews* in *China*; but either they must have been unskilful Relaters, or the *Chinese* bad Disciples, since *Kiang-ki*, it seems (e), made that Motion to be at the Rate of 1° in 50 Years; whereas *Ptolemy*, it is well known, made it 1° in 100 Years. It may be said indeed, that this Difference shews it could not be borrow'd from *Ptolemy*; but then it shews at the same time, that it could not be the Result of any Series of Observations; and that is as much, I think, as we need be concerned about. And this will appear yet farther, by remarking, that *A. D.* 460, it was made by *Tson-chong* 1° in 45 solar Years and 9 lunar Months (f). At other times it was made yet different still; but never, I think, from Observations of the Stars themselves.

(c) *Obs. ut sup.* Tom. 2, p. 25. (d) *Ibid.* p. 44. (e) *Ibid.*
(f) *Ibid.* p. 52.

selves. The Method, it seems, was by comparing the Places of the Solstices in their own Time, with their Places in the Reign of the Emperor *Tao (g)*, whom they supposed to have lived at a Time co-inciding with 2300 Years before *Christ*. Let us suppose them to have been right in this (a Thing we are by no means obliged to allow), yet, as they never appear capable of taking the *Solstices* with any tolerable Degree of Accuracy, we cannot wonder at any Mistakes we may meet with.

You have seen, Sir, all along, constant Mention made of *Chinese Calculations*; the very Word used by the Fathers themselves; tho' I am sensible that Term will be apt to lead the unwary into great Errors. For the most that can be made of their *Calculations* is nothing more than finding the Places of the Luminaries by Numbers (expressing their Periods and Parts of Periods), or, in other Words, by their mean Motions. For, as to the *Sun*, we are assured (*h*), they made his Motion one *Chinese* Degree in a Day, without knowing any thing of an *Equation* necessary to correct it. It was not till the third Century that they had any *Equation* for the *Moon (i)*; and *Tchang-tse-tsin*, about *A. D.* 550, as we have seen, was the first that used any for the *Planets*. It is observable, however, with regard to this Author, that there are no Writings of his extant (*k*); and therefore what is here said of him, may be nothing more than Report. Among an idle
vain

(g) *Observ. ut sup.* Tom. 2. p. 148.
(i) *Ibid.* p. 24. (k) *Ibid.* p. 58, 59.

(h) *Ibid.* p. 6.

vain People, unacquainted with critical Learning, round Assertions pass for Proof; and in this manner, beyond Question, the *Chinese* have frequently imposed upon credulous unskilful *Europeans*.

A. D. 618. began the Reign of the Emperors of the Race of *Tang*; and about this time other Western Strangers came into *China* (l). From them therefore the *Chinese* might learn what farther Improvements we may chance to meet with in their *Astronomy*, besides those for which they are assuredly indebted to the *Jesuits*.

It was observed but now, that in computing the Places of the heavenly Bodies, the *Chinese* at best knew nothing but their mean Motions: But in such Computations it is necessary to begin from some *Ratio*, or other: *European* Tables generally begin with the Commencement of the Christian *Æra*; the *Chinese* appear never to have known any. One *Epoch* indeed they have, but intirely imaginary, called *Chang-Tuen* (m), and which began some time or other at *Midnight*, at the *Moment* of the *Winter Solstice*, when the *Sun*, *Moon*, and 5 Planets, *Saturn*, *Jupiter*, *Mars*, *Mercury*, and *Venus*, were all in Conjunction, and the *Moon* without any Latitude.

This extraordinary *Epoch* began, according to these able Astronomers, 143127 Years before the *Winter Solstice*, in the Year before *Christ* 104 (n). What time this *Epoch* came first into Use is not known; but the *Fathers* think, and it must be acknowledged with

(l) *Obs. ut sup.* Tom. 2. p. 71, 72, 96.
(*) *Ibid.*

(m) *Ibid.* p. 16.

with great *Probability*, that it is not older than the burning the Books under *Tsin-chi-hoang* (a), or, as we have already seen, the Year before *Christ* 246. Should we place it however many Years later, or say that it never served any real Use at all, we may perhaps come much nearer the Truth.

For the *Chinese* Astronomers, as the *Fathers* observe (p), have spent an infinite deal of Time and Pains, in searching out this *Chang-Yuen*; and which has been carried up, they say, by some *two*, by others *three* Millions of Years beyond the Time it was above fixed at. But this shews to a Demonstration, that it is an *Epoch* purely fictitious; that, if it was real, it could only be of an *astronomical* Nature; and that they must be *foolishly stupid*, that should from thence collect, that the *Chinese* had any historical Memoirs of so ancient a Date. For the *Fathers* themselves allow, that the Opinion which ascribes to the World a Duration of some Millions of Years, is neither the general Opinion of the *Chinese*, nor of any ancient standing among them (q).

From what has been here offer'd, I think it is pretty evident, that, how ingenious soever the *Chinese* may be in Works of Art, their Talents do not lie towards *Mathematics* and *Astronomy*: For, was not this the Case, must it not be surprising, that having, as they say, so long a Series of Observations in the one Science, and of Professors in the other, they should never have been able to get beyond the first Elements of either?

It

(a) *Obs. ubi sup.* Tom. 2. p. 18. (p) *Ibid.* p. 17. (q) *Ibid.* p. 17.

It is not my Design to enter into any Controversy with the *Learned Fathers* of the *Society of Jesus*; the World hath been frequently indebted to them for their Philosophical Labours; and will be so again, when they shall have considered the *Chinese* History with proper Accuracy, and told us in what Manner they have been able to preserve Accounts and Observations of so ancient a Date. Public Libraries, it is allowed (r), they have none; nor doth it appear they ever had. Where then could Things so useless, as the Generality must have thought *astronomical Observations*, be repositèd? When intrusted to private Hands, they must have run great Risque of being destroy'd by Wars, by Fires, and in popular Commotions; which must frequently have happened in so long a Course of Years.

Let us suppose Things of this sort are of more Value to the *Chinese Commonalty*, or, if you please, their *Nobility*, than they are to the *European*; and that they would lay Hands on every thing they could meet with of the kind; and, when once in their Possession, would preserve it with a religious Exactness: But whence then comes it to pass, that it is *so difficult a Matter* in *China* to meet with Books upon these Subjects (s), to understand them when found, or to get any Assistance from the Natives towards understanding them?

But besides, are not Writings thus kept in private Persons Custody, unless carefully laid by, apt to be scribbled

(r) *Observ. ut sup.* Tom. 2. Pref.
S f f

(s) *Ibid.*

scribbled on by the Sciolists; so that it may be hard to distinguish many times the Text from what may be called the Comment? Is not this, in Fact, according to the *Learned Fathers* own Account (*t*), very frequently the Case? May not, by this means, a *Calculation*, if it must be so called, be mistook for an *Observation* made many hundred Years before? It is confessed (*u*), that *Martini* was imposed upon in this very manner; and it is much to be suspected that he hath not been the only one.

You see, Sir, that I have produced the *Jesuits* own Authority for every thing here offered. I designed to have cited their Words at Length, but that I found would have swell'd these Papers above the Size of a Letter. I am not conscious to myself of having misrepresented them; I am sure I have not done it wilfully; for I have nothing in my View but Truth. I am,

S I R,

March 2.

1747.

Your most obedient

humble Servant,

G. Costard.

P. S. The Title of the Work I have here cited, is, at length, *Observations Mathématiques, Astronomiques,*

(*t*) *Obs. ut sup.* Tom. 2. Pref. p. 103.

(*u*) *Obs. ut sup.* Tom. 2.

miques, Géographiques, Chronologiques, & Physiques, tirées des Anciens Livres Chinois, & faites nouvellement aux Indes, à la Chine & ailleurs, par les Peres de la Compagnie de Jésu.
It consists of three Volumes, printed at Paris.
1729, 1732.

XIV. *Part of a Letter from Mr. Turberville Needham to James Parsons M. D. F.R.S. of a new Mirror, which burns at 66 Feet Distance, invented by M. de Buffon F.R.S. and Member of the Royal Academy of Sciences at Paris.*

Dear Sir,

Read April 30. 1747. **Y**OURS of *December* came so late to hand, that I could not answer it till this Occasion. **** I have been at the King's Garden, and am just returned: I there learned, that this Morning they have been trying some Experiments with a new-constructed reflecting Mirror or Mirrors with Success: I knew indeed some time ago, that they had been upon the Design; and M. *de Buffon* had acquainted me with the theoretical Part of the Whole. I had even seen a Part of it executed; but as they had not then essayed it, I would take no notice of it: In one Word, it is *Archimedes* revived, and the Credit of Antiquity, in this Point, is

in some measure re-established. This Machine, for so I must call it, consists of 140 small plain Mirrors, each of about 4 by 3 Inches square; they are fixed at about a Quarter of an Inch Distance from each other, upon a large wooden Frame about 6 Feet square, strengthened with many cross Bars of Wood for the mounting of these Mirrors. Each of them have three moveable Screws, which the Operator commands from behind, so contrived, that the Mirror can be inclined to any Angle in any Direction that meets the Sun; and by this means the solar Image of each Mirror is made to coincide with all the rest.

There are in all, as I told you, 140 Mirrors; but they tried the Experiment this Morning with 24 only; for so many, and no more, were then ready for the Purpose: The Effect was, that, in very few Seconds of Time, a combustible Matter they had prepared with Pitch and Tow, daubed upon a Deal-Board, was set on Fire, and burn'd vigorously at the Distance of threescore and six *French* Feet. Judge now of the Effect 140 will produce; and whether the Invention may not be improved to the Height of all that has been advanced of *Archimedes* by the Ancients. The only Difficulty they found was, to make the solar Images of the Mirrors coincide; but this is owing to the yet Imperfection of their Method of mounting, which may be easily improved.

The Dimensions I have given in of the Mirrors and Frame were only guessed at from View, for I have not measur'd them; so you must not expect they will square or tally mathematically in the utmost Rigour. Nor, indeed, did I think it necessary

to

to do any more; for the Dimensions of themselves are purely arbitrary. If you have a clear Idea of the Construction of the Machine by this general Description, it is all I have aimed at.

XV. *Extract of a Letter from the Marquis Nicolini F. R. S. to the President, concerning the same Mirror burning at 150 Feet Distance.*

S I R,

* * *

Read April 30. 1747. YOU know that the Affair of *Archimedes* setting the *Roman* Fleet on Fire by means of Burning-Glasses, has been look'd upon as a Thing impossible and romantic. *Descartes* positively denied the Fact, which had been believed for so many Ages; and our modern Philosophers, after many Trials, and various Reasonings, have been of the same Opinion. But *M. de Buffon*, being asked if it might be possible to invent a *Phaometer*, or *Machine* for measuring the *Intensity* of *Light*, hath discovered by Trial, that *Light* was able to produce great Effects in a *Focus* at a great Distance, if one made use of a great Number of Disks, which would reflect so many Images of the Sun, and fling them all into one Place. He put together therefore a sort of *Polyedron*, consisting of 168 small Mirrors, or flat Pieces of Looking-glass, each 6 Inches square; by means of which, with the faint Rays of the Sun, in the Month of *March*, he set on Fire some Boards

of Beech Wood at 150 Feet Distance. By increasing the Numbers of Mirrors, he hopes to be able to do the same 900 Feet off.

His Machine has besides, the Conveniency of burning downwards or horizontally, as one pleases; and it burns either in its greater *Focus*, or in any nearer Interval, which our commonly known Burning Glasses have not, their *Focus* being fix'd and determin'd.

Perhaps this Machine may afford a Manner of measuring either Light, or the different Degrees of Heat of burning Bodies. The Difficulty is to find the Method of marking the Degrees, and of fixing a Point of Comparison; for the Point of kindling will not determine it; because that chiefly depends upon the greater or less Degree of Inflammability of different combustible Bodies.

Mr. *Maupertuis*, in a Letter to the *President*, dated at *Poitdam*, May 20. 1747. says, that his Friend *Buffon* has recover'd the Burning-Glasses of *Archimedes*; that with 168 plane Glasses, each 6 Inches square, he has melted a silver Plate, at the Distance of 60 Feet, and fired pitch'd Boards at 150. Each *Speculum* is moveable, so as, by the Help of 3 Screws, to be set to a proper Inclination for directing the Rays towards any given Point.

XVI. *Epistola Jo. Henrici Winkleri, Prof.
Lips. & R. S. Lond. S. ad Societatem Re-
galem Londinensem data, quæ continet
Descriptionem & Figuras Pyrorgani sui
Electrici.*

*Præses Illustrissime,
Viri Illustrissimi,*

Read May 7. 1747. **F**ACTA in Societatem vestram splendissimam mei cooptatione adeo præclarum munus mihi impertivistis, ut in agendis ejus partibus omnem vim ac operam adhibendam mihi esse videam. Cum enim ex vestra Societate per quamplurimos annos opera et scripta prodierint, quibus et propter rerum dignitatem, et propter auctorum celebritatem, et propter doctrinæ excellentiam, memoria perpetua constabit; facile intelligo quam arduum opus sit, quod inter viros, quorum fama universum literarum orbem peragravit, mihi suscipiendum est. Attamen vires animus colligit. Cum enim duobus abhinc annis ex primo meo de electricitatis natura libello scintillarum electricarum, quibus spiritus vini incenditur, præcipua phænomena in transactiones vestras descripta inferri curaveritis, Viri Illustrissimi; in eam adductus sum opinionem, fore, ut futura etiam opera mea, in qua continuanda non desistam, aliquid efficiatur, quod non displiceat. In præsentem vobis, Illustrissime Præses, Viri Illustrissimi, pro eximio, quo me ornastis, beneficio grates ago immortales, meque, quoad potero, exquirendis naturæ
viribus

viribus studium impensurum esse indefessum, et, si mihi obvenerit, quod notatu dignum videatur, hoc ad vos perscripturum esse pollicor. In libello *Germanico*, in quo anno superiori experimentum *Muschenbroekianum* explicaturus virium electricarum augmentum in vasis vitreis descripsi, § 30, organi alicujus, quo plures scintillæ simul oriundæ crepitant, mentionem injeci.

Hujus *Pyrorgani Electrici*, ut voco, imaginem, in figuris æneis dicti libelli non delineatam, in præfenti exhibeo TAB. IV. *Fig. 2.* Per medium annulum metallicum *ab* (*Fig. 1.*) pice repletum, cylindrus metallicus *cd* infixus est. Annulum diameter unum pollicem *Parissinum* et quatuor lineas æquat. Cylindrus utrinque ad distantiam unius pollicis prominet. Diameter annuli minor esse haud debet, ne cum cylindrulo communicata electricitas, si ad annulum metallicum pertingat, imminuatur. Ferrumine adjuncta est annulo *ab* furca metallica, cujus stylus cochleatus inditus est cylindro ligneo *ef*, (*Fig. 1.*) cujus extremitas inferior *f* ita formata est, ut per asseris alicujus fissuram pervadens huc illuc moveri, et ope cochleæ firmari queat. Ejusmodi cylindrum metallicum pici, qua annulus repletus est, infixum cum stylo cochleato, compendii causa, simpliciter *cylindrum electricum* nomino (*Fig. 1.*). Ex quatuor hujusmodi cylindris electricis pyrorganum electricum, *Fig. 2.* expressum, constat. Cylindri ita ponuntur, ut inter se habeant intervallum ad gignendas scintillas electricas sufficiens. Eas vero excitaturus pyrorganum juxta aliquod metallum *ab* ex filis sericis suspensum firmatumque ita colloco, ut inter metallum et cylindrum *c*, intervallum necessarium intersit. Ultimo cylindro *fg* in *g* alligo filum metallicum *h* in vas metallicum aqua plenum *i* pertingens. His ita ordinatis, simulac
metallum

metallum oblongum *ab* electricitate donatur, in quatuor intervallis scintillæ electricæ emicant tanto clarius splendentes, quanto majorem electricitatem communicant rotati frictique globi vitrei.

Ex quo libellus nominatus in lucem prodiit, duo ejusmodi pyrorgana electrica construxi, quorum alterum formam rotæ alatæ habet, alterum scintillis emicantibus septemtriones repræsentant.

Rotæ alatæ constructio, quam TAB. IV. *Fig. 4.* delineari curavi, est sequens. Orbi ligneo cavato seu annulo ligneo *dddd* sex alæ lignæ *cd* infixæ sunt. Totius annuli cum foramine diameter 13 digitos, et foraminis rotundi diameter 6 digitos complectitur. Alæ *cd*, quarum longitudo decem digitos comprehendit, fissuras habent, in quibus tres cylindri electrici hinc illinc moveri et firmari possunt. Prope alas infixas in annulo *dddd* foramina angulata formata sunt, in quibus singulis itidem cylindrus electricus statuitur. Itaque in rota alata sex ordines apparent, qui singuli quatuor cylindris electricis constant, observatis inter ipsos intervallis, quæ ad excitandas scintillas electricas profunt. In cujuscvis alæ extremitate *c* firmatur instrumentum metallicum *gf*, constans tribus partibus, quarum extremæ ad angulum rectum mediæ *g* junctæ sunt.

Ut vero instrumenta ista metallica rite applicari queant, in latere ad finem *c* alæ postico (anticum enim nomino, in quo cylindri electrici scintillant) fibula metallica affixa inter se et latus alæ posticum aliquod intervallum relinquit. Media fibula instructa est cochlea versatili *g*. Dicto intervallo pars brevior instrumenti metallici inditur. Pars altera extrema *h* cylindro quarto propinqua est; tantum vero ab eodem

distat, quantum debet, si inter istam instrumenti partem et cylindrum scintilla electrica excitanda est. Hinc pars altera brevior sub fibula pro lubitu moveri et firmari potest. Fibulis applicatum est et in partibus posticis alarum circumductum filum metallicum *ik*, cui, quando scintillæ electricæ suscitandæ sunt, in loco *x* aliud metallum *y* adjungitur in vas metallicum *s* aqua plenum pertingens. Electricitas, simulac cum cylindro primo communicatur, ad omnes cylindros transit.

Hinc necesse est, ut, si scintillæ exoriri debeant, instrumenta metallica descripta *gf* semper libera et vacua maneant electricitate. Quod fit metallo *y*, quod filo metallico *ik* adjunctum est, electricitatem in aquam propagante. Inter duo enim corpora æquali prædita electricitate nulla existit scintilla. Cum primo autem cylindro electricitas communicatur a malleo metallico *a* affixo axi metallico *bc*, qui in foraminibus rotundis duarum columnarum *de* lignearum versari potest præditus manubrio *f*. Hujus mallei apparatus *Fig. 5.* ostendit.

Columnæ *de Fig. 5.* ligno *mn* insistant, quod stylum *g* habet infixum operculo, quod vas vitreum *h* pice cum eodem copulatum tegit. Hoc vase vitreo ideo opus est, ut electricitas axi metallico *bc* ope alicujus fili metallici appensi data conservetur.

Vasis vitrei *h* fundus ope picis junctus est cum ligno, cui stylus adjunctus est, qui foramini longiori columnæ *ik* ita inseri potest, ut cochleæ *l* ope firmari queat, postquam vas vitreum debitam altitudinem nactum est. Hæc vero debet esse altitudo, ut axis *bc* in foramine annuli lignei *dddd* medium teneat. Cylindri autem electrici in annulo et alis *cd* ita

ita collocandi sunt, ut malleus a quocunque cylindro electrico primo, ad quem accedit, et cylindri a se invicem sufficientem scintillis electricis excitandis distantiam habeant.

In basi quadrata *mno p*, Fig. 4. in qua columna *ik* insistit, fissura est, in qua, quando malleus *a* satis per foramen annuli *dddd* prominet, columna *ik* cochlea stabilitur. Annulo *dddd* furca metallica applicata est infixæ columnæ lignæ *qr*, quæ similiter in fissura basis quadratæ *mno p* firma redditur. Axis metallicus *bc* per medium annuli foramen protensus a basi quadrata per altitudinem 21 digitorum abest. Quando igitur, axe *bc* electricitatem nactus, malleus alicui cylindro electrico primo appropinquat, quinque scintillæ secundum seriem rectam uno fere tempore exoriuntur.

Verso itaque sensim axe *bc*, ordines isti scintillarum per circulum in conspectum prodeunt. Scintillæ adeo claræ sunt, ut in luce diurna ad centum pedes videantur. Homo qui axem manubrio versat, ne electricitatem dissipet atque perdat, stare debet in materia electricitatem non propagante.

Electricos Septemtriones, TAB IV. Fig. 3. exhibet. In tabula *abcd*, quæ in columnæ *ef* fissura elevari et demitti potest, novem cylindri electrici ita collocari possunt, ut septem scintillæ eo ordine simul appareant, quo ipsi Septemtriones in cœlo nocte serena micant. Cylindro metallico primo eo tempore, quo existere debent scintillationes, adjungitur filum metallicum *b*, cui electricitas traditur. In tertii et quarti extremitatibus filum inflexum metallicum *i* annectitur, ut electricitas ad quintum et reliquos cylindros permeare valeat. Cylindro nono filum metallicum *k* applicatur in aquam in vase metallico *l* productum, quo

electricitas omnibus cylindris tributa in materiam, quæ eandem haud conservat, quantum opus est, propagetur.

Hæc quanquam nonnisi ad delectandos oculos spectare videantur, attamen vobis, Illustrissime Præses, Viri Illustrissimi, qui in ejusmodi delectamenti electricis vim naturæ mirabilem latere perspicitis, haud ingrata fore confido. Qua spe fretus, vestro omnium favori, quem impense veneror, me submisisse commendo.

Scribebam *Lipſiæ*,
die 31 Martii,
1747.

XVII. *Some Observations upon Gems or Precious Stones; more particularly such as the Ancients used to engrave upon, by Robert Dingley, Esq;.*

Read May 7. 1747. **G**EMS or precious Stones, of all Species, are sometimes found of regular Shapes, and with a natural Polish; and sometimes of irregular Shapes, and with a rough Coat. The first Sort may be consider'd as of the Pebble-Kind; and they are said to be found near the Beds of Rivers, after great Rains: The others are found in Mines, and in the Clefts of Rocks.

The Gems of the first Sort were what the Ancients most usually engraved upon: These are commonly called *Intaglio's*; and they are mostly of a long oval Figure, inclining to a Point at each End, convex as well on the engraved Face, as on the others, with a Ridge

Ridge running from End to End on the under Side, which is hereby, as it were, divided into two Faces; both which are also, tho' not so distinctly, parted from the upper Face, by another Ridge running quite round the Oval.

The Stone most commonly found engraved is the *Beryl*; that most frequently found next is the *Plasm* or prime *Emerald*; and then the *Hyacinth* or *Jacynth*. The *Chrysolite* is sometimes, but rarely, found engraved; as are also, but that very seldom, the *Crystal*, or *Oriental Pebble*, the *Garnet*, and the *Amethyst*.

Of the *Beryl* there are three Species; the Red, inclining to Orange-colour, transparent and lively; the Yellow, of an Ochre-colour; and the White, commonly called the *Chalcedon*, of the Colour of sheer Milk. These two last have less Life than the first.

The *Plasm* or prime *Emerald* is green, nearly of the Colour of stagnated Water; sometimes tolerably clear, but, for the most part, full of black and white Specks, and rather opaque.

The *Jacynth* is of a deep tawny Red, like very old *Port Wine*, but lively and transparent.

The *Chrysolite* is of a light-green Grass-colour, and is supposed to have been the *Beryl* of the Ancients, transparent, but not lively.

The *Crystal* or *Oriental Pebble* is harder and more lively than the common Rock *Crystal*; is of a silverish Hue, and but very little inferior to the white *Sapphire*.

The *Garnet* is of the same Colour as the *Jacynth*, but more inclining to the Purple, and not so lively.

The

The *Amethyst* is of a deep Purple, transparent and lively.

There were some other Species of Stones engraved upon by the *Romans*; but rarely before the latter Times of the Empire, when the Art itself was greatly upon the Decline.

All the before-mention'd Sorts of Stones are said to have been of the Produce of *Egypt*, or of the *East Indies*; and to have been brought from the Borders of the *Nile*, or of the *Ganges*.

Here follows a general Table of what are usually called Precious Stones.

The *Beryl*, is red, yellow, or white.

The *Plasm*, is green.

The *Jacinth*, of a deep tawny red.

The *Chrysolite*, of a light grass green.

The *Crystal*, or *Oriental Pebble*, of a silverish white.

The *Garnet*, of a deep red Claret-colour.

The *Amethyst*, purple.

The *Diamond*, white.

The *Ruby*, red or crimson-colour'd.

The *Emerald*, of a deep Green.

The *Aqua marina*, of a bluish Sea-green, like Sea-Water.

The *Topaz*, of a ripe Citron yellow.

The *Sapphire*, of a deep Sky-blue, or of a silver white.

The *Cornelian*, red or white.

The *Opal*, white and changeable.

The *Vermilion-Stone*, is more tawny than the *Jacinth*.

All

All these Stones are more or less transparent :
The following are all opaque :

The *Cat's-Eye*, brown.

The red *Jasper*, called also thick *Cornelian*, is of the Colour of red Ochre.

The *Jet*, black.

Agates, are of various Sorts.

The *Blood-Stone*, is green, veined or spotted with red and white.

The *Onyx*, consists of different parallel *Strata*, mostly white and black.

The *Sardonyx*, of several Shades of brown and white.

The *Agat-Onyx*, of two or more *Strata* of white, either opaque or transparent.

Alabaster, different *Strata* of White and Yellow, like the *Agate-Onyx*, but all opaque.

The *Toad's-Eye*, black.

The *Turquoise*, of a yellowish Blue inclining to green.

Lapis-Lazuli, is of a fine deep Blue.

Of most of the Species before-mention'd there are some of an inferior Class and Beauty. These are commonly called by Jewellers Occidental Stones: They are mostly the Produce of *Europe*, and found in Mines or Stone-Quarries; and are so named, in Opposition to those of a higher Class, which are always accounted Oriental, and supposed to be only produced in the more Eastern Parts of our Continent.

The

The *Onyx*, *Sardonyx*, *Agate-Onyx*, *Alabaster* of two Colours or *Strata*, as also certain Shells of different Coats, were frequently engraved by the Ancients in *Relief*; and these Sorts of Engravings are commonly called *Cameo's*. They also sometimes ingrafted a Head, or some other Figure in *Relief* of Gold, upon a *Blood Stone*.

Besides which there are some Antiques, mostly *Cornelians*, that are cover'd with a *Stratum* of White. This *Stratum* has by some been look'd upon as natural; but it was really a sort of Coat of Enamel that was laid on. This was used only in the Times of the lower Empire.

The Stones esteemed the best for engraving upon were the *Onyx* and *Sardonyx*; and next to them the *Beryl* and the *Facinth*.

The Ancients engraved most of their Stones, except the *Onyx* and the *Sardonyx*, just as they were found; their natural Polish excelling all that can be done by Art; but the Beauty of the several Species of *Onyx's* could only be discover'd by cutting.

The Merit both of *Intaglio's* and *Cameo's* depends on their Erudition, on the Goodness of the Workmanship, and on the Beauty of their Polish.

The antique Gems of *Greek Work* are the most esteemed; and next to them the *Roman* ones, in the Times of the higher Empire.

XVIII. Quadrantis *Astronomici* Muralis *Idea*
nova et peculiaris, multis incommodis qui-
bis hucusque usitati laborant liberati, Au-
ctore Christiano Ludovic. Gersten, R.S.L.S.

Read May 7. **I**NSIGNEM utilitatem arcuum parietibus in meridiani plano firmiter infixorum nemo nisi hospes in astronomiæ studio ignorat. Factum inde, ut nullum hodie facile detur observatorium supellestile sua instructum quod his careat; repertum tamen nullum dari murum tam solidum atque inconcussum, aut tam validam crassamque ferri et metallorum compagem, ut omnibus hisce organon hoc invariatur atque prorsus immobile respectu axis terrestris effici queat. Cogitavi itaque de novo artificio; arcum nempe muralem construendum propono telescopio et micrometro instructum, cujus requisita sint sequentia.

I. Ut quovis momento videri possit utrum planum instrumenti verticaliter positum sit: et,

II. Utrum perpendiculum per centium quadrantis et principium divisionis limbi exacte transcat.

III. Ut aberratio plani quadrantis a linea verticali corrigi possit, manente positione principii divisionis in limbo respectu perpendiculi: rursusque,

IV. Aberratio principii divisionis in limbo a perpendiculo corrigi possit, absque mutatione notabili positionis plani quadrantis respectu lineæ verticalis.

V. Ut pariter deviatio plani quadrantis a plano meridiei emendari possit, manente situ perpendiculari

U u u tum

tum plani quadrantis tum principii divisionis in limbo.

VI. Ut penitus expers sit variationis quam extensio metallorum per calorem et frigus producere potest. Denique,

VII. Ut facile possit rectificari uti dicunt, hoc est, Ut facile videri possit utrum linea ab objecto per intersectionem filorum in tubo ad oculum transiens sit exacte parallela lineæ per centrum quadrantis et divisionem quam regula monstrat transeuntis, atque ubi opus est facile in rectum situm collocari. Negotium alias admodum operosum et difficile.

Ut omnia ista obtineantur, fiat,

1. Fulcrum ferreum *aaa*, *ccb*, (TAB. V. *Fig. 1.* & 2.), cujus pars quæ muro applicatur *Fig. 1.* altera *Fig. 2.* delineata. Consistit ex norma ferrea *aaa*, et regula transversa *cc*, ad ipsam normam claviculis mallei ope firmiter consolidata. In *b*, normæ brachium horizontale est inflexum, ut in tergo prominere, habens insuper foramen rotundum horizontale, cujus usus mox inferius indicabitur, aliudque verticale minus cochleæ striis excavatum, ut cochlea mas, *m*, huic immitti possit.

2. In tergo fulcri ad brachium verticale normæ claviculis consolidatæ sunt ansæ duæ *ehk* et *dg*. Superior *ehk* desinit infra in cylindrum *h*, et cochleam marem *k*. Inferior *dg*, ad *g*, in superficie inferiori conice excavata; ut axis cylindri *h*, et apex conî *g*, sit in eadem linea, eaque plano normæ anteriori *aa*, *Fig. 2.* parallela postulo, nec tamen ut summa præcisio in his adhibeatur.

3. In ipsum murum plano meridiei utcūque parallelum immittantur, et idonea compage firmentur tres mutuli ferrei (*Fig. 4.*) *ah, gb, ce*; quorum duo, *ha* et *gb*, propemodum æquales, et in eadem linea perpendiculari existunt. Superior *ha* habet foramen cylindricum *h*, tantæ capacitatis, ut cylindrus fulcri *h* (*Fig. 2.*) eidem immitti possit. Inferior mutulus *bg* habet loco foraminis apicem conicum *g*, qui in cavum anfractus *g* (*Fig. 1.*) intrare possit. Distantia inter hos mutulos tanta, ut immisso in foramen *h* (*Fig. 4.*) cylindro *h* (*Fig. 1 et 2.*) et in cavum *g* (*Fig. 1.*) apice *g* (*Fig. 4.*) omne fulcri pondus apex *g* sustineat, libereque fulcrum horizontaliter volvi possit. Anfractus igitur superioris pars *c* (*Fig. 2.*) ipsum mutulum non premere, sed aliquantisper ab eo distare debet. Ne tamen ex cavo *g* (*Fig. 1.*) apex *g* (*Fig. 4.*) excidere aut vacillare possit, addatur cochlea formata ad marem *k*, cujus ope premendo inferiorem partem mutuli *ah*, fulcrum verticaliter ad apicem *g*, tantum deprimitur quantum sat est.

4. Ut cylindrus *h* (*Fig. 2.*) in foramine *h* (*Fig. 4.*) ab omni vacillatione penitus sit securus, addatur alia cochlea minor horizontalis *f*, vel duo ex oppositis plagis, cylindrum in foramine tangentes. Ut axis foraminis *h* et apex *g* (*Fig. 4.*) sit in eadem perpendiculari, hujus effectio nihil habet difficultatis in praxi, quoniam in ipsa muri structura hi mutuli sic disponi possunt. Primum inferior *gb*, deinde superior *ha*, ope perpendiculi cuspidati, cujus filum per axin disci æneji cavitatem foraminis *h* exacte replentis transire debet.

5. Tertius mutulus *ce* (*Fig. 4.*) consistit in crassa cochlea mari, longe satis ex muro prominente. Fo-

ramen *b* (*Fig. 2.*) fulcri tantæ capacitatis, ut hanc cochleam transmittat, ipsius cochleæ superior pars adenta, ut habeat planum horizontale, cui cochlea *m* (*Fig. 2.*) inniti possit. Immisso itaque cylindro *b* (*Fig. 2.*) in foramen *b* (*Fig. 4.*) et cavo *g* anfræ *d*, (*Fig. 2.*) applicato ad conum *g* (*Fig. 4.*) additaque cochlea foemina ad *k* (*Fig. 2.*) eademque applicata ad partem inferiorem mutuli *ab* (*Fig. 4.*) ad cochleam mutuli *ce* applicetur cochlea foemina quædam plana, orbicularis et in margine dentata, ut eo melius clavi quodam expresse ad id confecto circumagi possit, adigatusque usque ad partem mutuli *c* propemodum. Deinde horizontaliter volvendo ipsum fulcrum, cochlea *e* (*Fig. 4.*) foramini brachii horizontalis fulcri *b* (*Fig. 2.*) immittatur, circumvolvetur cochlea *m*, eo usque donec planum cochleæ crassæ mutuli *e* attingat, et ipse mutulus adeo ponderis fulcri partem aliquam sustineat. Deinde addatur adhuc alia cochlea foemina plana et dentata ad eandem marem *e* (*Fig. 4.*); agatur usque ad planum brachii horizontalis normæ, id quod in nullum alium finem flexum, nisi ut hanc cochleam foeminam sic recipiat, ne suspensioni quadrantis in fulcro obstat, atque ut eo major concedatur longitudo cochleæ crassæ mutuli *e*. Sic fulcri pars *b* (*Fig. 2.*) nititur hoc modo verticaliter in cochleam mutuli *ec* (*Fig. 4.*) et in positione azimuthali servatur per duas descriptas cochleas foeminas dentatas mutuli *te*. Quodsi igitur aliquid in positione plani muralis aberratum, istud semper quo tempore libuerit, mediantibus hisce cochleis dentatis, corrigi potest. Me non monente, benignus lector intelliget foramen brachii horizontalis *b* (*Fig. 2.*) satis amplum et ovalis figuræ esse debere, ne angustia motui azimuthali obstat.

6. Fulcri pars anterior (*Fig. 2.*) tres rursus habet mutulos *n*, *o*, et *p*. Primus *n* est ad formam cubi aut parallelipedi, nisi quod superficies ejus superior sit semicylindriace excavata. Secundus *o* unci formam habeat (*Fig. 3.*) specialiter depictam. Tertius *p* nihil aliud est nisi cochlea mas prominens. Cuncti ad normam quoad fieri potest optime consolidati.

7. Quadrans ipse ut ex solido sit metallo postulo. Facies ejus anterior exhibetur TAB. VI. *Fig. 1.* Crassities sit congrua, limbusque quadrantem idonea quantitate excedat. Ad planum limbi rectificandum adsit regula composita *kk*, ex duabus, quarum una *rr* perpendiculariter insistit ad planum alterius *kk*, sic ut in neutram partem facile flecti possit. Regulæ *rr* acies rectitudinem habeat perfectam. exhibeatque lineam rectam quæ sit in plano superficiei limbi anterioris. In tergo per cochleas hæc regula quadranti firmatur, sicuti ex *Fig. 2.* apparet. Quodsi enim hæc regula plano limbi in *m* et *n* bene coincidit firmata ad centrum alia regula rectilineari examinatória, facile patebit utrum limbus et acies *rr* regulæ in eodem sint plano, per consequens limbi planum recte se habeat. Nam ex punctis *m* et *n*, nonnisi unica recta duci potest, quæ per hypothesin in acie regulæ *rr* revera existit. Per lineam rectam vero *mn*, et punctum *a*, nonnisi unicum planum duci potest. Quodsi igitur regula examinatória ad *a* affixa ubique aciem regulæ *rr* et limbum quadrantis exacte tangit, planum limbi in plano trianguli *amn* necessario esse debet. Post examen et correctionem hæc regula *rr* superflua est, adeoque, si liber, adimi potest.

8. In tergo quadrantis (*Fig. 2.*) duo sint sustentacula orichalcea, *ab* et *ef*, ad ipsius quadrantis superficiem per cochleas probe firmata. Sustentaculum *ab* in *b* habeat foramen ovale; in lateribus sint duo cochleæ *c* et *d* cuspidatæ. Foraminis parietes superne sint convexæ, ut immisso mutulo *o* (*TAB. V. Fig. 2.*) in hoc foramen, pars cava *a* mutuli (*Fig. 3.*) replatur, cuspidisque cochlearum horizontalium *c* et *d* (*TAB. VI. Fig. 2.*) partem mutuli aut unci convexam inferiorem (*TAB. V. Fig. 3.*) ex duabus oppositis partibus stringant, et sic omnis vacillatio excludatur.

9. Alterum sustentaculum *ef*, in *e* cochleis ad planum quadrantis affixum, ad *f* habet foramen rectangulare, quod ingrediuntur cochleæ mares verticales *h* et *i*, quarum usus est sequens. Immisso mutulo sive unco *o* (*TAB. V. Fig. 2.*) in foramen sustentaculi *ab* (*TAB. VI. Fig. 2.*), mutulus *n* (*TAB. V. Fig. 2.*) quoque immittitur in foramen rectangulare sustentaculi *ef*, et cochleæ *h* apex, qui hemisphærice convexus esse debet, in cavo mutuli *n* (*TAB. V. Fig. 2.*) subsistat, et sic per motum cochleæ *h* situs quadrantis elevando et deprimendo parumper est variabilis. Foramen *f* in hunc finem satis amplum esse debet; quando tamen semel situs conveniens per cochleam superiorem determinatus, tunc vacillatio quadrantis non proficua, ideo per cochleam inferiorem *i* firmiter ad mutulum constringitur.

10. Duobus hisce sustentaculis quadrans in situ debito quidem teneri, ac, ubi opus est, corrigi potest, respectu principii, divisionum in limbo, ut istud nimirum perpendiculo congruat per centrum quadrantis ducto: Sed requiritur ulterius, ut et ipsum quadrantis planum sit perpendiculare. Ut id efficiatur, adsint

addant duo cochleæ foeminæ planæ orbiculares, cochleam marem *p* (TAB. V. *Fig. 2.*) complectentes; harum prima eaque dentata, mari *p*, applicetur antequam quadrans mutulis fuleri sit applicatus. Postea vero, ubi quadrans sustentaculis *cd* et *fe* (TAB. VI. *Fig. 2.*) mutulis *o* et *a* (TAB. V. *Fig. 2.*) pender, atque cochlea mas *p* in foramine *c* (TAB. VI. *Fig. 1* et *2.*) existit (quod sat amplum et ovalis figuræ esse debet, ut concessa sit variatio quædam situs, per cochleam ansæ superiorem *b* (TAB. VI. *Fig. 2.*) efficiendam); tunc tergum quadrantis sustinetur cochlea foemina orbiculari dentata, ad marem *p* (TAB. V. *Fig. 2.*) applicata, paulo superius descripta, facies vero per alteram cochleam foeminam orbicularem, clavi quadam, foraminulis nonnullis in hunc finem terebratis intrusa, circumagendam. Planum itaque harum cochlearum foeminarum, sat amplum esse debet ne in ipsum foramen quadrantis ingrediantur. Et sic his duabus cochleis non modo valide constringitur quadrans ab utraque parte, sed etiam, ubi opus est, retro vel antrosum promovetur, quoniam suspensio in mutulis *o* et *n* (TAB. V. *Fig. 2.*) huic motui non obstat. Quoniam tamen nimia longitudo cochleæ *p* obest, consultum crit cochleam anteriorem foeminam tali figura construere, quam sectio (TAB. VII. *Fig. 2*) exhibet, ubi margo *ab* faciem anteriorem quadrantis tangere, collum *cd* vero ipsum foramen *c* (TAB. II. *Fig. 1.*) ingredi debet.

II. Centrum quadrantis (TAB. VI. *Fig. 1.*) *a* cylindrice excavatum est, ut clavum regulæ admittat. in tergo quadrantis, medianibus cochleis affixa est lamina *mn* (*Fig. 2.*) foramen habens quadratum *m*, centro respondens; sitque hoc quadratum, circulo foraminis

foraminis *a* (*Fig. 1.*) inscriptum, aut paulo minus. Lamina *mn* (*Fig. 2.*) crassitiem habeat congruentem, sitque dupliciter ad normæ modum flexa, desinatque in facie in partem *b* (*Fig. 1.*) a plano quadrantis factis distantem, ut stylum tenuem centrum clavi pungentem, filumque perpendiculi sustentem teneat. Ipsum clavum (*Fig. 3.*) specialiter delineavi, in eoque est *a* caput, *b* cylindrus cavum centri quadrantis, et regulæ exacte complens, *c*, quadratum foramini *m* (*Fig. 2.*) immittendum, *d* cochlea mas, cui conveniens scœmina applicanda.

12. TAB. VII. *Fig. 1.* exhibet quadrantem cum regula et perpendiculi apparatu: *pr* est linea superficiei quadrantis inscripta, per centrum, si continuata fuerit, transitura, qua divisionum in limbo principium est. *hikg* est parallelipidum eo modo, sicuti figura indicat, excavatum, cochleis ad planum quadrantis affixum, quo filum *mo* perpendiculariter imminet lineæ *pr*, quod normæ accuratioris ope nullo negotio perficitur. Alterum filum *ik* situm habet ad planum quadrantis parallelum, ad tantam tamen exacte distantiam, quanta est altitudo centri in clavo *c*, sive punctum suspensionis fili supra superficiem quadrantis. Addatur tertium, secundo prorsus simile in latere opposito parallelipedi *hgv*, cujus delineationem scenographiæ leges verant.

13. Ipsum filum stylo subtili centrum clavi *c* pungente suspensum, ex pilis capitis humani consistet, pondusculum *f* semiuncie facile sustentens, et in cavo parallelipedi *hikg* libere oscillet. Fila minora parallelipedi *mo, ik*, &c. etiam ex iisdem pilis consent, habeantque crassitiem æqualem. Usus eorum hic est; ut facile cognoscatur positio quadrantis

tis respectu perpendiculari *ef*. Nam per filum *mo*, ad lineam *pr* collimando, exactum datur oculorum iudicium, utrum *pr*, principium divisionum in limbo, cum perpendicularo *ef* conveniat. Rursusque per filum *ik* ad alterum oppositum collimando, videri potest utrum planum quadrantis sit perpendicularo parallelum. Quod si loco parallelepipedi duo ponticuli, hæc tria fila sustinentes substituantur, idem finis brevius adhuc obtinebitur, cumque spatium oscillationis fili *ef* exiguum sufficiat, dispositio horum trium filorum talis esse potest, ut per vitra convexa collimatio fieri possit; quod commodum accidet, quibus obtusior oculorum acies. Cochleæ foeminæ *b* margo orbicularis, adeoque et ipsa cochlea mas fulcri *p* (TAB. V. *Fig.* 2.) quam complectitur, sic ultra superficiem quadrantis promineat, ne oscillationem fili *ef* impediat, aut, si mavis, istarum cochlearum et foraminis in quadrante locus extra oscillationis fili spatium assignandus. Quodsi observatorii structura patiatur, ut ab horizonte ad *Zenith* stellarum detur aspectus, tunc regula ad lineam *pr* accessum debet habere liberum, adeoque parallelepipedum *bikg* paulo inferius in appendicem collocandum, aut superius paulo in brachium verticale, ipsaque appendix incisionem convenientem habere debet.

14. Progredior nunc ad ipsam regulam, quæ quidem TAB. VII. *Fig.* 1. quoad maximam partem scenographice delineata, atque literis *nnnn* designata extat, insuper tamen habita iusta magnitudine et proportionem partium, quam in omnibus hisce figuris negligendum duxi, quoniam sic multo distinctius mentem meam figuris spectabilioribus explicare potui. Hæc regulæ structura, quoniam ea plane peculiaris, spe-

cialius nunc mihi explicanda. *nnnn* est planum regulæ circa clavum *c* volubilis. Contra incurvationis periculum munita est alia regula *ddd*, perpendiculariter ad planum *nnn* afferruminanda, aut alio idoneo modo consolidanda. Divisiones limbi indicantur in apertura, aut fenestra regulæ *xx*; ibique aut per filum extensum, aut per aciem vel marginem interiorem aperturæ, aut per divisionem quam vocant Nonianam, prout libuerit. *qq* est telescopium. Quod si nunc ponamus regulam exacte gradum 0 vel 1 altitudinis in limbo monstrare, nihil erit quod in organo desiderares, nisi ut linea ab objecto per decussationem filorum telescopii ad oculum pergens, sit exacte parallela lineæ per centrum quadrantis et gradum 0 vel 1, altitudinis in limbo transeuntis. Sed cum prima vice tubus talem in modum regulæ consolidari nequeat, ut huic conditioni satisfiat, postulo eum sic regulæ connecti, ut in situ, quo TAB. VII. *Fig. 1.* depictus est, motum aliqualem habeat non solum in altitudinem, sed etiam in partes azimuthi. Motus in altitudinem perficitur per cochleas *uu*, motus azimuthalis vero per cochleas *ww*, quæ omnia tamen specialius et distinctius TAB. VIII. delineata extant.

15. Motus in altitudinem efficitur sequentem in modum. Tubo loco convenienti afferruminatur pes aut ponticulus TAB. VIII. *Fig. 6.* depictus; basis *bck* est plana et rectangularis, ad *b* et *c* *k* crassities metalli minor. Pars *i* perpendiculariter ad basin afferruminata, huic connectitur altera pars volubilis *namh*, mediante stylo *h*, juncturas utriusque partis transcendo. In *a* pars volubilis excavata, tali curvedine ut tubum recipiat stanno in hunc canalem afferruminandum. Basin hujus pedis *b'c'k* ad planum regulæ deprimunt, duo
 frustra

frusta orichalcea, quorum figuram *Fig. 7.* sistit, et quos *depressores pedis* voco. Habent autem hi depressores duo foramina *k* et *m*, ad recipiendas cochleas, et duos apices *h* et *i* foraminulis in plano regulæ in hunc finem terebratis intrudendos. Melius nunc omnia ex ichnographica delineatione horizontali *Fig. 2.* intelligentur, ubi *aa* tubi pars, *bc ee* pes, *ee* basis pedis, *bc* pars volubilis tubo afferruminata, *ff* duo depressores pedis, *gg* cochleæ mares, matrices in regula excavatas ingredientibus, et hoc modo pedem valide prout libuerit ad planum regula deprimentes. *Fig. 5.* exhibet delineationem verticalem, in eaque *aa* tubi pars, *ee*, *ff*, cochleæ depressorum; ipse pes sub tubo latet: *g, h*, sunt duæ cochleæ mares, quarum apices planæ ab utraque parte pedem stringunt immotumque servant, et ubi situs horizontalis tubi elevando vel deprimendo mutandus, id facile per revolutiones harum cochlearum perficitur: sed tunc cochleæ depressorum pedis *ff*, *ee*, non nimis fortiter pedem ad planum regulæ deprimere debent.

16. Sequitur nunc motus ad partes azimuthi. Quod *Fig. 9.* delineatum, vocemus *tabulam plicatam*. Consistit ex plano rectangulari orichalceo *kfgh*, cui ad angulos rectos insistat aliud planum *abcf*, cujus latera tamen *af* et *bc* sint in curvedine circuli, ex centro foraminis *q* ducti, margines *af* et *dbcg* sunt ad angulos rectos excavati, prorsus eodem modo sicuti basis pedis (*Fig. 6.*). Cui respondere debent duo depressores similes ei quem *Fig. 7.* sistit, nisi quod curvedinem ipsi *bc* convenientem habere debeant. Planum *kfgh* aperturas habet rectangulares *mnn* et *osp* pro recipiendis appendicibus tubi quas mox describam. In *Fig. 8.* *aa* est tubi pars, *ee* ta-

bulæ plicatæ pars verticalis, mediante clavo *b*, ad planum regulæ firmata, atque circa clavum volubilis, *cc* et *dd*, depressoires tabulæ, atque earum cochleæ. *bb* est sectio partis tabulæ horizontalis, quæ *Fig. 9.* literis *kfgb* indicatur. *gg* sunt appendices peculiæres, tubo afferruminatæ; desinunt in cochleas mares, et pars earum foraminibus rectangularibus *mn* et *op* (*Fig. 9.*) respondet, sic ut secundum longitudinem *mr* aut *os* possint intervallo quodam apto hinc illincque promoveri, et tamen mediantibus cochleis fœminis *kk*, interpositis antea orbiculis *ii*, quando libuerit ad ipsum planum tabulæ valide firmari. Quodsi igitur TAB. VII. *Fig. 1.* per cochleas *uu* contra pedis parietes applicatas telescopium attollitur aut deprimitur, tabulæ complicatæ clavus istud permittit, relaxatis paulisper cochleis depressoires *xx*. Rursus quando ad motum azimuthalem peragendum, appendices *ww*, relaxatis cochleis fœminis hinc illincque, per consequens ipse tubus promovetur, pedis junctura id quoque permittit. Quodsi appendices *g, g,* (*Fig. 8.* TAB. VIII.) transverse valido satis metalli frusto connectuntur, hic motus azimuthalis unius cochleæ artificio et lentus et securus effici potest.

17. Præterea habet regula appendicem peculiarem tergum limbi radentem. Constat TAB. VIII. *Fig. 1.* et 2. ex parte normaliter inflexa *Am*, plano regulæ validissime consolidata et altera volubili *ko*, stylo *m*, juncturis suis cum immobili *Am* connexa, in parte volubili orbiculus *i* *Fig. 2.* circa cylindrum *q* in cochleam desinentem versatilis tergum limbi radit, atque adversus istud constringitur clauetro *ns* cochleis *ssss* (*Fig. 1.*) firmato. Mota regula ad quamlibet deinde divisionem firmatur ope cochleæ *v*, cui objicitur lamina tenuis

tenuis *ut Fig. 2.* ne apex metallum limbi excavet, contactum immediatum impediens.

18. In vitris telescopii nullam requiro quam vocant centrationem, hoc est, ut maxima crassities sit in medio vitri, negotium alias tædiosum et operosum. Hoc tantum postulo, ut vitra præprimis objectivum constantem habeant situm in tubo, in quem, si ad abstergendum forte amoveantur, facile rursus restitui possint. Angli artifices, passim vitra ocularia telescopiorum præprimis reflectentium, cylindris cochleæ helicibus incisus firmiter infigunt, quo deinde aptis matricibus tubo immittunt. Quod si igitur helices cochleæ sunt bonæ, et præterea signum quoddam efficitur in cylindri margine, alii in cavo tubi facto respondens, necessario vitrum eundem situm servabit vel centies amotum, modo sic tubo rursus inseritur, ut signum signo respondeat.

19. Micrometrum, TAB. VIII. *Fig. 4.* habet collum, cujus margo in octo æquales partes lineis ad centrum convergentibus, scrupulose, aut si plura fila verticalia requiruntur in alias quascunque partes divisus. Ad has divisiones fila sive serica sive metallica facile applicantur, firmananturque in collo aut ceræ ope, aut subtilioribus claviculis, ex buxi ligno aut metallo confectis. *a* est prominentia quadrangularis, habens in parte opposita similem aliam, sed aut majorem aut minorem: ambarum crassities æqualis esto aut paulo minor crassitie tubi cui immittitur. Ipse tubus *Fig. 3.* delineatus, in *d* habeat crenam rectangularem, prominentiam annuli *a* (*Fig. 4.*) absque vacillatione recipientem. Ipse annulus micrometri bene cavo tubi conveniat, intrususque tubo parietes interiores ejus bene contingat. *b* est vitrum oculare, a filis micrometri

micrometri debito modo distans. *f* foramen oculare, operculo cochleato *b* incisum. Remoto hoc operculo aliud substitui debet vitris fuligine obductis munitum. Ut micrometri filum horizontale semper sit in eodem situ, aut si disturbatum, ad debitum situm rursus redigi queat, tubus major telescopii *aa* (*Fig. 5.*) desinat in cuspidem *i*. Inseto deinde tubo oculari *b*, et in debitum situm redacto, linea in superficie externa tubi *b*, ad ductum marginis horizontalis cuspidis *i* ducatur, et cochleis minutioribus postea hi duo tubi *a* et *b* ad se invicem firmentur. Quodsi micrometrum postulatur, filo mobili instructum, tunc structura convenienter mutanda.

20. Nunc ad rectificationem regulæ progrediendum: Fiat ex ligno sicco et solido asser, superficies habens horizontalem ejusdem ferme longitudinis et latitudinis cum regula. In extrema superficiei horizontalis parte sit verticaliter erectus clavus æneus, in cochleam superne desinens, cavitatem foraminis centralis exacte replens. Alterum extremorum planum habeat, aut tabulam æneam parallelipipedalem prominentemque. TAB. VII. *Fig. 3.* *bb* denotat partem asseris, *fegk* tabulam æneam prominentem, cochleis *eeee*, probe ad ipsum asserem firmatam, sic tamen ne capita cochlearum in superficie tabulæ promineant. Ipsa metalli superficies superior sit probe polita, congruat cum plano superiori asseris, sitque in eo ducta linea subtilis *ab*, quæ, si continuata fuerit, axin clavi transeat. Sint porro duo parallelipipeda orichalcea *cd* duabus cochleis, ad ipsam tabellam firmata, sed in apta ab extremo *gk* distantia, ex sequentibus mox colligenda. Singula infra habeant quatuor apices cylindricos foraminulis in tabulam terebratis intrusos, ut
situs

situs eorum sit quam firmissimus; duo hæc paralle-
 lipeda se mutuo exacte contingant, et planum con-
 tactus sit perpendicularare ad lineam ab . Quodsi igitur
 divisiones limbi quadrantis monstrantur per mar-
 ginem interiorem aperturæ xx (TAB. VII. *Fig. 1.*),
 aut filum quoddam per aperturam istam extensum tunc
 regula quadrantis primum sic imponitur asseri, hori-
 zontaliter primum collocata ejus superficie superiori,
 ut clavus verticalis asseris per foramen centrale trans-
 eat, telescopio sursum spectante, et sic remoto paral-
 lelipedo d (*Fig. 3.*) margo aperturæ xx , divisiones
 indicans, (*Fig. 1.*) aut filum extensum exacte applice-
 tur ad superficiem perpendiculararem parallelipedi c
 (*Fig. 3.*); quo facto margo aperturæ aut filum exten-
 sum erit in plano ad lineam ab perpendiculari. Porro
 firmata regula ad tabulam fg ope cochleæ v (TAB.
 VIII. *Fig. 1. et 2.*) telescopium cum ipso asseri diri-
 gatur ad objectum quoddam remotum, immobile;
 notetur in eo punctum, quod per decussationem filorum
 obtegatur, immotusque maneat in ista positione asser.
 Postea invertatur regula, removeatur parallelipedon c ,
 et in pristinum locum restituatur d (TAB. VII. *Fig. 3.*);
 margo aperturæ xx vero (*Fig. 1.*) sive filum applice-
 tur ad planum perpendicularare parallelipedi d . Cum-
 que hoc statu minima pars regulæ asseri incumbat,
 adeoque necesse sit ut reliquum cum telescopio præ-
 ponderet, fulcrum quoddam cochleis instructum as-
 seri combinandum. Applicata ergo situ inverso re-
 gula ad idem planum perpendicularare lineæ ab (*Fig. 3.*)
 denuo ad objectum per telescopium collimandum,
 videndumque an punctum decussationis filorum sit in
 eodem objecti puncto ac antea. Hic casus si extite-
 rit, nulla correctione opus habet regula. Nam cum
 immota

immota fuerit recta *ab*, per centrum clavi regulæ transiens, idemque objecti punctum per telescopium tam in situ recto quam inverso apparuerit, necesse est ut linea ab objecto per decussationem filorum ad oculum transiens parallela sit lineæ *ab*. Sed cum hic casus vix unquam prima vice sic existere possit, sed semper fere aliud objecti punctum fila decussantia tangant, tunc aberratio potest esse aut in altitudine, aut in azimutho, aut in utrisque simul. Utroque casu telescopii positio per cochleas *uu* et *ww* TAB. VII. *Fig. 1.* ad dimidium aberrationis angulum corrigenda, quantum sc. oculorum iudicio hoc affirmari potest. Deinde regula rursus in situ recto modo, uti antea, imponenda, noviterque per telescopium objectum contemplandum, mox rursus invertenda, videndumque an idem punctum appareat. Si secus, telescopii positio denuo dimidii erroris quantitate corrigenda. Examen hoc toties repetendum, quoties differentia adhuc aliqua appareat. Re perfecta omnes cochleæ *dd*, *cc*, *kk* (TAB. VIII. *Fig. 8.*) et *ee*, *ff*, *gh* (*Fig. 5.*) firmiter adstringendæ, ut telescopium in eo statu permaneat. Cui hæc multiplex regulæ in asserere inversio, quæ tamen sat brevi tempore absolvi potest, adhuc tædiosa nimis videbitur, is aut micrometrum addat cum filo mobili, aut, quod ego malle, tabulam albam *bcde* (TAB. V. *Fig. 6.*) quæ duas fascias nigras *hl*, *gf*, habeat ad angulos rectos, situ horizontali et verticali sese decussantes, ad congruam distantiam disponat, et sic deinceps asserem cum telescopio situ recto imposito sic dirigat, ut punctum decussationis fasciarum sit in puncto decussationis filorum in tubo. Immoto asserere, sed inversa tamen regula, prope tabulam adsit focus, mandata observatoris per signa data.

data exequens, habeatque iste duas alias fascias nigras *km* et *no*, quas ad nutum observatoris situ ad *gf* et *hl*, parallelo (facile structura aliqua peculiari in affere sic efficiendo) et quidem *no* horizontaliter, *km* vero verticaliter moveri possit. Quodsi enim inversa regula in affere punctum decussationis cadit in tabulæ punctum, v. gr. *g*, tunc socius utraque fascias *km* et *no* successive sic disponat, ut filum verticale tubi cadat in fasciam *he*, et horizontale tubi in fasciam tabulæ *km*. Hoc peracto, intervalla *fo* et *kh* ope circini bisecanda, atque fasciæ in tabula motu parallelo in ista bisectionis puncta collocandæ. Tunc immoto affere regula cum telescopio in situm rectum rursus restituitur, atque telescopii positio tamdiu per cochleas corrigitur, donec punctum decussationis fasciarum in tabula coincidat cum puncto decussationis filorum in tubo. Quodsi prima vice inversa regula et tubo ad tabulam collimatur, secunda vice vero in situ recto, tunc puncto decussationis in tabula notata, atque debito loco collocatis fasciis mobilibus, error in eodem situ regulæ corrigi potest. Et sic tubi situs respectu regulæ erit talis, ut vel in recta vel in inversa positione telescopii, idem objecti punctum in tubo conspiciatur, per consequens regula quadrantis erit rectificata.

21. Quod si fieri nequeat quin hæc regula paulo evadat ponderosior, pro commoda ejus ad astra directione, duobus modis et huic incommodo mederi potest. Primus est quem dudum *Flamstedius* sextanti suo adhibuit, atque in *Historia Cælesti* descripsit. Cochlea nimirum perpetua in regulæ appendice versatili margo exterior limbi quadrantis denticulis congruis incisus raditur. Sed tunc apparatus appendicis *sq* (TAB. VIII. Fig. 2.) non omittendum suadeo,

ut regulæ planities plano limbi bene sit contigua. Nam in sextante murali *Petropolitano*, qui a *Rowlejo* præstantissimo sui temporis artifice confectus est, hoc vitium deprehendi, ut margo aperturæ *xx* (TAB. VII. *Fig. 1.*) divisiones limbi ibi indicans, satis bene quidem limbum stringat, reliqua vero pars regulæ nimium a plano limbi distet, unde telescopium vacillat, vel validissime ad marginem adstricta cochlea. Sed totum hoc artificium nimis operosum mihi videtur, nec satis commodum in observationibus. Nam cochleæ revolutionibus limbi divisiones aut examinare, aut corrigere, aut comparare, nullo modo satis tutum mihi videtur; pondus regulæ sublevare, facilioremque tubi ad astra directionem hoc artificio efficere, id admodum nimis operosum. Mallem ego alio uti centies simpliciore. Sit TAB. V. *Fig. 5. abc* quadrans muralis, *ad* regula; in *m*, centro *a* quadrantis verticali statuatur axis vectis ferrei, *gmf*, sic ut brachium *mf*, et ejus revolutio, sit quamproxime in plano quadrantis; alterum *mg*, in alio parallelo, atque distanti. Longitudo *mf* sit dimidium circiter longitudinis regulæ *ad*, longitudo *mg*, vero 3 vel 4 pollicum. Angulus *gmf* punctorum nempe rotationis *m*, et suspensionum *gf*, sit rectus. Ad *f*, catenulæ vel funiculi ministerio connectatur regula, sic ut *mf* sit æqualis et parallela *ae*. Ad *g* alligetur funiculus *gh* horizontaliter ad extremum conclavis extensus; ibi trochlea *h* fultus, tandemque pondere *k* appenso ad terram propemodum demissus. Quodsi enim hoc pondus *k* regulæ sufficiens efficitur, in quocunque situ, ea vel demissa vel in altum sublata manebit. Parum obstabit, quod centrum gravitatis regulæ nostræ a puncto suspensionis distans futurum sit, quoniam
pressio

pressio elastri in tergo limbi inæqualem gravitatis actionem satis temperabit.

22. Restat nunc ut ostendam organum descriptum cunctis requisitis suis satisfacere.

I. Cum fila i, k , (TAB. VII. *Fig. 1.*) et alterum ipsi simile in opposita parte parallelipiedi situm habeant ad planum quadrantis parallelum, eandemque distantiam ac punctum suspensionis c , facili negotio collimando patebit, utrum filum perpendiculi sit in plano horum filorum, adeoque planum quadrantis in positione verticali. Quod erat unum.

II. Quoniam filum mo est in plano lineæ pr , idque perpendiculare ad planum quadrantis, collimando per mo et pr facile dignoscitur, utrum perpendiculum sit in hoc plano, per consequens principium divisionis pr sit in plano verticali. Quod erat secundum.

III. Quoniam quadrans suspensus est duobus punctis, primum in unco o (TAB. V. *Fig. 2.*) deinde in cavo mutuli n , motum ejus verticalem nihil impedit nisi duæ cochleæ foeminæ, marem p complectentes. His ergo cochleis situs plani quadrantis ad perpendiculum corrigi potest, cumque hac correctione brachium horizontale quadrantis non inclinetur, sequitur quod hæc correctio sit independens, a situ horizontali quadrantis. Quod erat tertium.

IV. Uncus (TAB. V. *Fig. 3.*) non modo in a est cavus sed simul est convexus, ut sectio ejus ab sit circularis: hæc unci figura igitur non obstat quo minus quadrans in mutulo n per cochleam b (TAB. VI. *Fig. 2.*) nonnihil attolli aut deprimi possit. Proinde cum positio principii divisionis dependeat simul a positione brachii horizontalis, evidens est, per motum cochleæ b , positionem principii divisionis respectu perpendiculi corrigi posse.

V. Cum ipsū fulcrum ferreum habeat motum aliquem horizontalem in mutulis *ab* et *gb* (TAB. V. *Fig. 4.*) axitque rotationis sit perpendicularis, sequitur, quod reliquis omnibus manentibus deviatio plani quadrantis a plano meridici corrigi possit. Et si vel maxime axis rotationis fulcri non exacte sit perpendicularis, nihil tamen impedit, quo minus observator inclinationem hujus axis, ad quam partem ea tendat, explorare possit, et sic deinceps pro re nata correctiones suas instituire. Quod erat quintum.

VI. Quadrans ipse ex uno et solido est metallo. Quod si igitur extenditur aut contrahitur calore aut frigore, similis semper sibi manebit. Nec suspensio ejus obstat extensioni. Nam in fulcro mutulus *n* (TAB. V. *Fig. 2.*) canalem habet horizontalem, in quo cochleæ apex *b* (TAB. VI. *Fig. 2.*) hæret, et foramen *c* (*Fig. 1.*) satis amplum pro tam parva extensione aut contractione quam calor aut frigus producit, nec cochlearum foeminarum, istud foramen in facie et tergo quadrantis tegentium, brachiumque constringentium, superficies plana huic obstat. Quod erat sextum.

VII. Denique quoad rectificationem, ea jam satis ex ipsis præceptis pater. Nemo non intelliget, eam faciliorem expeditioremque esse hucusque usitatis; cumque asser pro rectificatione regulæ constructus asseruari possit, quolibet tempore observator absque magno labore regulam suam denuo examinare potest. Ergo organon requisitis suis satisfacit.

Cæterum quoniam rarissime in ædificiis constructis dantur muri in plano meridiei existentes, vel saltem loca hisce muris construendis apta, arcus murales hucusque semper ædificium expresse in hunc finem, dispositum desiderarunt. Sed quilibet facile intelliget nostrum artificium quocunque ferme loco esse applicabile.

plicabile. Nam si, *v. gr.* in tali apertura quæ januarum est, duo mutuli *ab*, *gb*, (TAB. V. *Fig.* 4.) in murum immittuntur, tertius *ec*, murum contiguum præcise non requirit; sed in ferri quadam compage, longe de muri plano prominente, valide fatis firmari potest.

Multa præterea sunt quæ in hoc organo aliter aut forte melius disponi possent; sed longus nimis forem omnia cogitata mea in medium hoc loco prolaturus. Sufficiat artificium scopo congruens monstrasse, idque non ingratum penitus astronomis fore confido.

XIX. *Observationes duæ Alberti Haller, Prof. Med. Gotting. R. S. S. Lond. S. Fabricæ morbosæ in Cadaveribus repertæ.*

OBSERVATIO I.

Read May 7. 1747. **I**N foemina quadragenaria reperi venam cavam inter renalis sinistræ originem, et inter iliacas venas, enormiter angustatam, ut vix quidquam transmitteret. Aliquid tamen polyposi duri sanguinis in ejus cavitate, quæ inter crassas membranas arctissima fuit, repertum est. Vena vero spermatica dextra enormiter dilatata, unciali diametro venæ cavæ locum subiit, et sanguinem, exclusum a via solita, reddidit ureteris venæ, alioquin in sano corpore exiguæ, ortæ ab iliaca dextra.

Rarissimi hujusmodi coalitus exemplum aliud reperi in JOHANNIS RHODII *Mantiss. Anatom. Obs.* XXI.

Ex utroque constat, etiam in maximis truncis vatorum corporis humani morbos subnasci posse, et obstructionem in venis vere dari; et canales minimos, quando sanguinis, solitis viis exclusi, impetus eo vergit, patentissimos reddi posse.

OBS. II.

IN fœmina decrepita, quam centum annorum ætate esse dictabant, non tamen ita firma fama, ut eam sequi tuto liceat, ætatis summæ aliqua vestigia repcri.

Tota corporis fabrica durior fuit, vel cultro iudice. Glandulæ conglobatæ sanæ, sed similes fere renalis carnis firmitati; nervi præduri; cellulosa tela ubique vix scissilis; costarum cartilaginee nondum ossæ, nisi supremam velles, quæ cum sternone, nexu vix ullum discriminis vestigium relincente, conferruminata erat. Sed in ea costa id non rarum est.

Verum in arteria magna multæ mortis causæ fuerunt. Amplissima primum aorta, qua ex corde prodit, ut quinque unciarum et linearum duarum esset ambitus. Deinde aperto hoc, non aneurysmate quidem, sed amplissimo tamen sinu adparuit.

1. Valvulas cordis arteriosas partim induratas, partim etiam petrosas tumoribus varias esse, perinde uti COWPERI fere figuræ demonstrant [*Myolog. reform.* T. XI.]; reliquæ valvulæ venosæ et arteriosæ cordis vix mutatæ.

2. In arteria aorta, tum ad cor, tum in thorace, in abdomine denique, membrana interna undique lacera, quasi scabendo in eminentes cristas liberas, fluctuantes, mutata, tamquam ulcere aliquo consumpta esset. Hæ squamæ passim ossæ erant, alicubi etiam petrosæ, et acervi tophaceorum granorum plerorumque vasorum ex aorta oriundorum ostia obsidebant. Membrana musculosa sana fuit, tum externa, ut vitium omne in intima federet.

3. In hypogastricis, iliaticis, pelvis, arteriis, et iis quæ ex pelvi ad nates exeunt, plurimæ crustæ ossæ, subflexiles tamen, in quas mutata erat interna harum
arteriarum

arteriarum membrana, ita tamen adhærentibus fibris carnis, ut passim calculosæ squamæ ductibus transversis inscriberentur. Nihil tamen hic petrosi. In omnibus arteriis corporis prædura et figurata teretia sanguinis crassamenta, suo canali tamen minora.

4. Vesicula fellea flava bile vix amara plena, et calculis ad viginti, exiguis, angulosis, quorum unus ita obsidebat ostium ductus cystici, propius paulum cholidochæ, quam prima cystici valvula, ut bilis, contra quam solet, ex vesicula præmendo expelli non posset. Dulcedinem in bile, quando in calculos coivit, plerumque reperire soleo.

Hæc fere fuerunt, quæ observationem mererentur, et demonstrant arteriarum internam membranam ab ictibus repetitis cordis tandem partim indurescere, partim inter ossificata spatia rumpi, sic debilitari truncum aortæ, et ad aneurysmata reddi pronum. Demonstrat etiam in ipso sanguine ubique terrarum veram calculosam circumvehi, nec in renalibus solum viis deponi, sed ibi hære et congeri, ubi ruptæ sunt levissimæ membranæ vasorum, et adtractio terrearum molecularum ad asperas inæquales superficies major est.

XX. *A Letter from Mr. David-Erskine Baker to the President, concerning the Property of Water Efts in slipping off their Skins as Serpents do.*

S I R,

London, May 13. 1747.

Read May 14.
1747.

AS several little Particularities in the Production or Changes of Animals may be found of considerable Service to assist us in

in the Knowledge of Nature, and consequently are not unworthy a Philosopher's Notice, I take the Liberty to lay before you some Observations on the common *Lacerta aquatica*, *Water-Lizard*, *Newt* or *Eft*; a Creature which most People, tho' without any good Reason, have imagined to be venomous and mischievous, and, from a groundless Aversion have avoided and neglected much more than it deserves.

The Animal I speak of is to be found in the Spring, and during the whole Summer Season, in most Ditches and shallow standing Waters throughout *England*, and is, I believe, unknown to very few; but, lest it should possibly be mistaken for any other Creature, I beg Leave to lay a Picture of what I mean before you.

When fully grown, it is about 6 Inches in Length; The Head is like that of a Frog, with a Couple of fine large Eyes: It has four short Legs, the two foremost having four Toes, and the two hindmost five, resembling the Feet and Toes of a Frog; but not at all webbed, as the hindmost Feet of a Frog constantly are: The Tail is very thin and flat, and lies not horizontally, but stands up in a perpendicular Position, and serves as a Rudder to direct it in swimming. It is amphibious, but lives mostly in the Water, wherein, tho' it can swim, it most frequently crawls about at the Bottom, rising to the Surface only now-and-then, with a wriggling Motion, to throw out a Bubble of stale Air, and take in a fresh Quantity of new. There is some Difference in their Colour, but the Back is usually of a light brown, and the Belly yellowish spotted with black Spots. In
Winter-

Winter-Time they are seldom or never seen, and therefore may be supposed to retire into Holes, and lie torpid there, as Frogs, Snakes, and many other Creatures do.

Having several Times kept these Animals for many Months together in Glass Jars, and watched them very attentively from Day to Day, I am able to speak with much Certainty as to the wonderful Manner of their putting off their Skins without making the least Hole or Breach therein; a Circumstance which has occasioned me to give you the Trouble of this Paper.

It has long been known, that most of the Serpent-Kind put off, or, as we commonly term it, cast their Skins at certain periodical Times; tho' we are very little acquainted with the Manner of their performing this Work, since it is commonly done in their retiring Places, where we can seldom get a Sight of them; nor should we indeed know that their Skins are changed at all, did we not often find the Skins they have cast off. But from this little Lizard, which I have more than once carefully attended during the whole Operation, a reasonable Guess may be formed as to most other Kinds; and as it is a Creature easy to procure, may be kept in a Jar of Water for many Months, and the Intervals between the Periods are so short (for they shed their Skins every Fortnight or three Weeks), it is in every Body's Power to see with his own Eyes what I am now going to describe.

A Day or two before the Skin is to be changed, the Animal appears more sluggish than usual, takes no notice of the Worms you give it, which at other times it devours greedily; the Skin in some Places appears loose from the Body, and its Colour not so

lively as it did before; and thus it continues till the great Work of putting off the old Skin is to be performed. It begins this Work by loosening with its fore Feet the Skin about its Jaws (which, when open, are wider than any Part of its own Body) and pushes it backward gently and gradually both above and below the Head, till it is able to slip out first one Leg, and then the other; which when it has done, it proceeds to thrust the Skin backwards as far as these Legs can reach; it is then obliged to rub its Body against Pebbles, Gravel, or whatever else it can meet with, till more than half its Body is freed from the Skin, which appears doubled back, and covering the hinder Part of the Body and the Tail. When the Business is thus far done, the Animal, turning its Head round to meet its Tail, takes hold of the Skin with its Mouth, and setting its Feet thereon, by degrees pulls it quite off, the hind Legs being drawn out as the fore ones were before.

If the Skin be then examined it will be found with its Inside outwards, but not having the least Hole or Breach; that Part which cover'd the hind Legs seeming like Gloves that are turned without pulling out the Tips of the Fingers, tho' intirely perfect and unbroken. The Coverings of the fore Legs remain within the Skin. They do not however put off the Coverings of their Eyes along with the Skin, as some Snakes are found to do; for the Skin of this little Creature has always two Holes at the Places where the Eyes have been.

It is very entertaining to observe it whilst engaged in this necessary Work, which sometimes takes up
near

near half an Hour, after which it appears full of Life and Vigour, as well as very sleek and beautiful.

These Observations have been made under the Inspection of my Father, and some other curious Friends, who are Witneses of the Facts here mentioned. The Drawing (TAB. X. *Fig.* 1. 2.) added to this Account, and intended to represent one of the Animals getting rid of its Skin, may, it is hoped, assist to make the foregoing Description more fully understood. When the Skin is come off, if it be not taken away soon, it is very common for the Creature to swallow it whole, as it does all its other Food; and if it takes in the Head-Part, as frequently is the Case, the Tail-Part, being filled with Air and Water, becomes like a blown Bladder, and proves so unmanageable that it is very diverting to see the Pains it costs to discharge the Air and Water, and reduce it to a fit Condition to be got down its Throat.

Many Creatures of very different Kinds put off their Skins and Shells at certain Periods. All Serpents are supposed so to do; the Skins of several Kinds being oftentimes found whole. Crabs, Lobsters, Cray-fish, Shrimps, and probably most or all of the crustaceous Fishes, cast their Shells from time to time; and if one may guess of the rest by the fresh Water Shrimp, which I have kept several times and observed, their Shells are put off without any other Breach than one, longitudinally, in the Middle of the Belly Part, thro' which the Body, Tail, and Claws are pull'd out, and the Shell left in a Manner whole.

[Of the Insect Tribe, every Caterpillar has three or four Skins before its Change into the *Aurelia*

State, in which the Place of creeping out is a little below the Head. The Spider throws off the Skin or Shell three or four times, getting out of it by a Rupture underneath, and leaving every Claw, and even the horny Covering of his *Forceps* intirc. Even the little Mite casts its Skin also at several short Periods, and nearly in the same manner. But I fear I have been already too tedious; and therefore begging your Pardon, subscribe myself, with all possible Respect,

S I R,

Your most obedient humble Servant,

David Erskine Baker.

A Skin of the Water Lizard cast off in the above manner waits on you herewith, as represented in (TAB. X. *Fig. 2.*) the extreme Thinness of it makes it impossible to be got out of the Water, stretched out and dried, and therefore it was necessary to be preserved in Spirits.

I beg Leave likewise to shew you one of the living Animals, as figured in TAB. X. *Fig. 1.*

A Remark by the Publisher.

WM. Oliver, the Viper-catcher, mentioned in No. 443. of these *Transactions*, made a Present to the *Royal Society* of a female Viper big with Young, which was kept alive in common

common green Moss, in a Box with a glass Cover. She brought forth several young ones, who slipp'd off their Skins, and the outward Membrane of their Eyes along with them, in six Weeks after their Birth; and they shed them again two Months after: But being then put into Spirits of Wine to preserve them, they were killed; but may still be seen in the *Museum* of the *Society*. They first loosen the Skin about the Mouth, and so slip it off backwards, by wriggling themselves thro' the Entanglement of the Moss: For some of the Skins were torn, and Parts stuck in the Moss.

C. M.

XXI. *An Improvement of the Celestial Globe,* *by Mr. James Ferguson.*

Read May 14.
1747.

ON the *Axis* of the Globe, (TAB. X. *Fig. 3.*) above the Hour-Circle, is fixed the Arch *A* at one End by the Screw *D*, so as to leave sufficient Room for turning the Hour-Index occasionally: The other End at *B*, being always over the Pole of the Ecliptic, has a Pin fixed into it, whereon the Collets *C* and *B* are moveable by their Wires *F* and *G*, when the Screw *E* is slackned, and may be made fast at Pleasure by this Screw; so that the turning of the Globe round will carry the Wires round with it, shewing thereby the apparent Motions of the Sun and Moon by the little Balls on their Ends at *H* and *I*. On the Collet *C*, in which the Sun's Wire is fix'd, there is also fix'd the circular Plate *L*, whereon the $29\frac{1}{2}$ Days of the Moon's Age are engraved, which have their Beginning just below the Sun's Wire; consequently the Plate *L* cannot be turned without carrying the
Sun's

Sun's Wire along with it; by which means the Moon's Age is always counted from the Sun: And the Moon's Wire being turned so as to be under the Day of her Age on this Plate, will set her at her due Distance from the Sun for that time. These Wires being Quadrants from *C* to *H*, and from *B* to *I*, must still keep the Sun and Moon directly over the Ecliptic; because the Center of their Motions at *C* and *B* is perpendicularly over the Pole of the Ecliptic in the *Arctic* Circle. But, because the Moon does not keep her Course in the Ecliptic, as the Sun appears to do, but has a Declination of $5\frac{1}{2}$ Degrees on each Side of it in every Lunation, she is made to screw on her Wire as far on both Sides as her Declination or Latitude amounts to. For this Purpose *K* is a small Piece of Pasteboard, to be applied over the Ecliptic at right Angles; the middle Line *oo* standing perpendicularly thereon. From this Line there is $5\frac{1}{2}$ Degrees marked on each Side upon the outward Limb; which reaching to the Moon, makes her to be easily adjusted to her Latitude at any time.—*N. B.* The Horizon should be supported by two semicircular Arches, instead of the usual Way of doing it by Pillars; because the Arches will not stop the Progress of the Balls, when they go below the Horizon in an oblique Sphere.

To rectify the Globe. Elevate the Pole to the Latitude of the Place; then bring the Sun's Place in the Ecliptic to the brazen Meridian, and set the Hour-Index to XII at Noon: Keeping the Globe in this Position, slacken the Screw *E*, and set the Sun directly over his Place in the Meridian; which done, set the Moon's Wire under the Day of her Age for that

that time on the Plate *D*, and she will stand over her Place in the Ecliptic for that time, and you will see in what Constellation she then is. Lastly, fasten the Wires by the Screw *E*, and the Globe will be rectify'd.

To find the Rising and Setting of the Sun and Moon, with their Amplitudes on the Horizon.

The Globe being rectify'd as above to the given Time, turn it round in the usual Way, and you will see the Sun and Moon rise and set for that Day on the same Points of the Horizon as they do in the Heavens. The Times of their Rising and Setting are shewn by the Hour-Index, which likewise shews the Time of the Moon's passing over the Meridian. If you want to see to greater Exactness the Rising and Setting of the Moon, find her Latitude for that Day by the *Ephemeris*; and as it is South or North, screw her so many Degrees from the Ecliptic, measuring them by the Pastboard *K*, applying it to the Ecliptic as above mention'd; and then turning the Globe round you will see the Time of the Moon's Rising and Setting by the Hour-Index, and her Amplitude on the Horizon for that time, as it is affected by her Latitude, which will sometimes be very considerable.

This may be very useful, especially in giving Lectures upon the Globes; because a large Company at some Distance will easily see this Sun and Moon going above and below the Horizon as they rise and set, and likewise their Appulses to different fix'd Stars: Whereas in the usual Way, when there is only the Sun's Place in the Ecliptic shewn, it is not easy
for

for any one to keep his Eye upon that Part of the Ecliptic as the Globe is turned round, unless it be in some remarkable Circle of Longitude; and it is not very easy to know the Moon's Place, unless at her Conjunction, Opposition, or Quadratures.

This simple *Apparatus* shews all the Varieties that can happen in the Rising and Setting of the Sun and Moon, which are very curious, especially about the Poles, where the Sun is present for one Half of the Year, and absent for the other Half; the Moon in Winter shining constantly without setting from the first to the third Quarter, in the Sun's Absence; and in Summer the full Moon is never seen at the Poles; for she sets at the first Quarter, and rises not till the third, save what may happen on account of her Latitude.

All the *Phænomena* of the Harvest-Moon become very plain by this additional Part; and in making some Trials I find, that, to some Places of the Earth, the Moon will not differ above an Hour in her Rising for fifteen Nights together, but will differ sometimes 23 Hours in her Setting, within the Compass of that fifteen Days; and for the next fifteen she will set within an Hour of the same Time, and differ 23 Hours in her Rising. This is taken in round Numbers, but may be consider'd with more Exactness by those who are better acquainted with the celestial Motions. I shall only add, that the Places of the Earth where these *Phænomena* happen, are those lying under the Polar Circles.

XXII. *The Case of a young Child, at Houghton in Huntingdonshire, born with all its Bones displaced: Communicated* Febr. 8, 1746. *by Mr. Edward Davis, Surgeon at Huntingdon, to Dr. Herman Heineken in London.*

Read May 14.
1747.

BEING desired to see a young Child, who, they told me, was born with all his Bones displaced, I visited it, and found both the *Radius* and *Ulna* of the right Arm, with the Bones of the *Carpus* and *Metacarpus*, also the Fore-finger and Little-finger of the same Hand, all dislocated. The *Radius* and *Ulna* of the left Arm were dislocated, and receded from each other; likewise the Fore-finger and Little-Finger of the same Hand. The *Os Femoris* of the right Leg was dislocated very oddly, and laid downwards, so that one might feel the End of it: The *Patella* laid high up the Thigh; and the *Tibia* and *Fibula* at their Union with the *Os Femoris* were also dislocated, and receded very much from each other. The right Leg, the *Tibia* and *Fibula*, at their Union with the *Os Calcis*, also the *Os Calcis*, and the *Tarsal* and *Metatarsal* Bones, likewise most of the Toes. The left Leg, the *Fibula*, with some of the *Metatarsal* Bones, and and some of the Toes. The Head, upon Examination, likewise is very curious: The *lambdoidal* Suture is ossified all round, and rises with a Prominency half an Inch high: The occipital Bone has several Risings, which feel like several *Exostosis's*; and the two protuberant Sides of the occipital Bone are enlarged

enlarged to a prodigious Degree, and unite with each other, but leave a Dent between them which feels like a Suture. They are enlarged, I believe, to six Inches long, and three broad : It is all ossified ; the Midwife and Nurse say it was soft at first : The rest of the Head appears very well.

This Child is seven Days old : I, have reduced the dislocated Bones, tho' some with great Difficulty ; for the Ends of the Bones and Cartilages seemed to be all ossifying ; and there seems to be an universal *Anchylosis* coming on. I could not reduce the right Foot well ; it was all ossified, with the Bones displaced, and the *Extensor Pedis Pollicis longus* was contracted, and had drawn the Foot almost round. The Jaw-Bone was also dislocated, which the Midwife could easily put in its Place, and the Chin-Stay supported it very well ; that is, almost well, only apt to slip out on one Side. The Midwife and Nurse say, they could easily, for the two first Days, put all the Bones in their Places with Ease, but they continually fell out again.

The Mother receiv'd a Fall a Fortnight before Delivery, and she fancies the Bones were displaced with the Fall, tho' she did not hurt herself : But whether it is from thence, or from some Vice in the Fluids, I shall not determine. If it were not for several *Exostosis's* and *Anchylosis's* in several Parts, I should have imagin'd the Child (tho' so young) were rickery ; but for the above Reason it cannot be that. The Child seems at present lusty and strong, but I think will soon be otherwise ; the Woman is lusty, and walks out about her Business, tho' but a Week ago deliver'd ; and she has six Children besides, all very healthy.

healthy. — I intend, if the Child dies, to do what I can to get it, and make a Present of it to the *Royal Society*.

XXIII. *A Dissertation on the Situation of the ancient Roman Station of Delgovitia in Yorkshire; by John Burton, of York, M. D.*

Read May 28. 1747. **T**HE learned Antiquarians have hitherto been greatly at a Loss to find the Place where the *Delgovitia* of the *Romans* really stood; some supposing it at one Place, and some at another.

My worthy and learned Friend, Mr. *Francis Drake*, in his excellent History and Antiquities of *York*, has given us every thing which has hitherto been wrote in Support of the Claim made by each Place to the Honour of rising out of the Ruins of that ancient Town; together with his Reasons for fixing that Station at *Londesburgh*; all which I beg Leave to recapitulate in as few Words as possible, and to make some few Remarks thereupon, before I proceed to shew where I think *Delgovitia* really was.

There are three Places where the Site of *Delgovitia* has been fix'd at; viz. *Weighton*, *Godmanham*, and *Londesburgh*. See the Map in TAB. X.

The Reasons offer'd for fixing it at *Weighton* are three;

First, From the supposed Derivation of the Name.
Secondly, From something like a *Tumulus* being at the East End of the Town. And

A a a a 2

Thirdly,

Thirdly, Because the Distance from *Derventio* agrees with the Itinerary.

As to the first, the learned *Cambden* would have *Delgovitia* to be derived from the *British* Word *Delgwe*, which signifies the Statues or Images of the Heathen Gods; therefore, as some Persons would have *Weighton* to be derived from something of the like Cause, upon no other Foundation, than that *Weighelberg* in *Germany* is noted by *Conrad Celtes*, says *Dr. Gale*, as a remarkable Town in those Parts.

Another Person (a) says, that *Delgovitia* was called *Devovicia*, or *Delvovicia*; from whence take *Vic*, and add the *Saxon* Termination *Ton*, there is something like *Wighton*; especially when we consider that the *Saxon* U and W were sounded alike.

Both these (supposed) Proofs for *Weighton* will drop, when we consider, that an easier and much more probable Derivation of it may be found out; the Name being intirely *Saxon*, and is plainly derived from the *Saxon* Word *weg*, or *yaeg*, *Via*, *Stratum*, a Road or Street; and from the Verb *pegan*, *ire*, *transire*, to travel: The Termination *Ton* is obvious to all; so the *Belgic* or *High-Dutch* *Wech*, *Wegh*, *Weghe*, are the same with our *Way*, and signify the same Thing. This is most likely, because *Weighton* now stands at the Conjunction of several Roads, which here meet, and run from thence by *Kexby-Bridge* to *Tork*, and thence may be called *Weighton*, or *Way-Town*. The *Roman* military Ways, both from *Prætorium* and from *Lindum*, took

(a) In the Chorography of *Britain*.

a different Course, and went by *Londesburgh*, as I shall shew in the Sequel; and the old Road being turn'd this Way, a new Town sprung up, which took its Name from the Occasion of altering the Road.

The second Reason offer'd to prove *Weighton* to have been *Delgovitia* is, because Mr. *Horfeley* observed something like a *Tumulus* at the West End of the Town. This alone is so weak an Argument to prove this Place a *Roman* Station, that I shall take up no Time in endeavouring to confute it. I must also remark, that there are not the least Remains of any *Roman* Road leading to *Weighton*; which there always are to any known *Roman* Station.

The third Reason offer'd, is, because this Place agrees so near with the Distance from *Derventio*, as mention'd in the Itinerary; but this Argument must fall, when I shall shew in the Sequel, that it is not true in Fact, having had the Road measured.

There is no other Reason offer'd, why *Delgovitia* was where *Godmanham* now is, than that this latter Place was said, by Venerable *Bede*, to be *Locus Idolorum*, or a Place of Idols. Mr. *Burton*, in his *Itin. Anton.* seems to lay a Stress on the *quondam Idolorum Locus*, and says, It may allude, as well to *Roman* Idols as *Saxon*: But this is too far strain'd; and we may justly enough conclude, that this was a Temple neither of *Roman* Structure nor Worship, but a Place dedicated to *Saxon* Idolatry; such a one as is described in *Verstegan*, inclosed with a Hedge instead of a Wall: For we find the curious Mr. *Drake* made a very strict Enquiry. and could not perceive the least Remains of any Ruins; meeting with nothing

thing but Holes and Hills, where Chalk or Lime-Pits had been made.

There being not sufficient Proof offer'd to fix *Delgovitia* here, I shall now proceed to examine what Mr. *Drake* brings, to prove that *Londesburgh* may claim this Honour, he being the first Person who has attempted to shew it; and I must own, has brought much more substantial Reasons to support his Opinion, than have been mention'd in Behalf of either *Weighton* or *Godmanham*. These are five in Number; *viz.*

First, From the Name of the Place, *i. e.* *Burgh* or *Brough*.

Secondly, The Distance from *Eboracum* and *Derwentio* will answer the calculated Miles in the *Itinerary*, as well as *Weighton*.

Thirdly, Because the *Roman* Road lay that Way.

Fourthly, Because *Roman* Coins are found there.

And

Fifthly, Because there are Repositories for the Dead often found.

As to the first; That it might derive Part of its Name, *viz.* *Burgh*, from a Fortrefs on Land, I agree to; and very likely there might have been some Guards kept there; because it stands so high, that it commands the Prospect of the whole Country from thence to *Brough*, where the *Romans* used to ferry over to and from *Lincoln*; as appears by the military Road on both Sides the River. Here might, I say, have been a Guard kept, to see that no Enemy came on that Side to surprize them in their Station; and which indeed seems to be the more confirmed by the Coins found here, as well as the Number

of human Bones. Tho' this last is no certain Proof; because a Battle or Skirmish might have been fought near or upon this Place; which indeed seems to have been the Case, so many Bodies being found together; yet, when join'd with other Circumstances, it helps strongly to confirm his Opinion. But I have not heard of any old Foundations, Ruins, or *Roman* Pavements having been discover'd in or near this Place.

The second Reason offer'd is, because it will answer the calculated Miles in the *Itinerary*. This Place comes nearer the *Itinerary* than *Weighton*; but this Point I shall wave discussing, till I come to treat of that Place where I think *Delgovitia* stood.

The third Reason is, because the military Way led thither from *Brough*. This, tho' a Proof that the *Romans* passed and repassed where that Place now stands, yet is no Proof of its being a *Roman* Station.

The fourth Reasons I have already spoke to under the first Head here offer'd; so I shall now proceed to prove, I hope, where *Delgovitia* really stood.

I shall not spend much Time in speaking of the Wisdom of the *Romans*, either in their military or political Capacity: Some few Things however, tending that Way, I am obliged to mention, in order to shew their Motives for fixing a Station in the Place I hint at, and then I shall endeavour to bring my Proofs that that Station was their *Delgovitia*.

Among others, these were strong Reasons for keeping the several Stations; *viz.* to guard the Passes, to keep the Country in Awe, and to have a considerable Number of Men together, to prevent a Surprise from any foreign Invasion, and sometimes to secure a Supply of Provision.

I don't

I don't know any Part of the County of *York* that required so strong a Guard as this Place (strong it has been, is evident from the Plan of the Camp); because it is so situated, that *York*, for want of this Station, might have been sooner surpris'd, either by any foreign Enemies, who might have made a Descent upon them, from either the *Humber*, or *German Ocean*; or from an Insurrection of the Inhabitants of the *East-Riding*; both which are by this Station sufficiently guarded against. It likewise gave them an Opportunity of receiving and defending their Recruits, either from *Rome*, or from any other of the Southern Parts of the Island; who could either come by Water, or cross the River from out of *Lincolnshire*.

As the guarding their Passes was one strong Reason for fixing Stations, any Person, by looking on and examining the annexed Map, (*see TAB. X.*) and the Draught of the Camp, (*see TAB. IX.*) will easily at one View, see why that Place must be preferable for such a Station; for it is just at the Angle where four *Roman* military Ways meet; so that, by guarding sufficiently that one Pass, they secur'd all the four Roads; which, had they been station'd either at *Weigh-ton*, *Godmanham*, or *Londesburgh*, they could not with the same Men have done. This will be more evident, by drawing a Line of the *Roman* Road on the Map of *Yorkshire* from *Brough Ferry* on the River *Humber* thro' *Londesburgh* Park to *Malton*, and from *Stamford Bridge* to *Pattrington* near *Spurnhead*; and then you will find that the Roads cross each other betwixt or near *Millinton* and *Wartre Priory*.

Moreover the Country itself, by Nature, helped, with only little Art, to make their Camp at that
time

time almost impregnable, the Hills being from 60 to 90 Yards perpendicular in Height, and their Sides very steep, which are very apparent in the Plan.

From one Part of this Camp they could see a great way towards *Malton*, and all the Way down the great Vale of *York*, from near *Hambleton-Hills* to *Howden*, and from *London'sburgh* (where I doubt not but they kept a Watch-Guard) they could see all the Way from *Howden* to *Brough Ferry*. Hence they could not be surpris'd from the South-East, South-West, or North-West Quarters; therefore they had only chiefly the Eastern Sides to fortify; and how they have done that, the Plan of their Camp (TAB. IX.) will better shew than I can describe.

Another Reason for their fixing here is very evident; because, at the Foot of the Hill, not 100 Yards from where the *Roman* Pavement was, there are two Springs of fine clear Water, which, united, form what they call the *Beck*. These Springs never fail, even in the hottest and driest Summers (a rare thing to be met with upon the *Wolds*) and there is not another Spring within two Miles of that Place, but what is either quite dried up, or greatly diminished in a dry Season; insomuch that at this Day, in some Seasons, the People are obliged to drive their Cattle several Miles hither for Water.

From this Situation their Army cou'd never want Provisions, having a free Communication either by Land or Water, with the Southern Parts of the Island.

All that Part of the Plan of the Camp (TAB. IX.) marked *a. a. a. a.* describes deep Valleys; from the Bottom of which to the Top of the Hills are in general

B b b b

from

from 60 to 90 Yards in perpendicular Height, and the Sides are very steep. All along the Hills, from Vale to Vale are *Roman Works*, represented in the Plan at *b. b. b. b.*; so that nothing could pass that Way, without the Knowledge and Consent of the Guards. It must also be observed, that, of all the Works, those guarding the Parts toward *Bridlington* are the strongest; they being from 4 to 6 Ditches in Breadth, each of which are 10 or 12 Yards broad.

At the Places marked *c. c. c. c.* in the Valleys, were Watch-Guards, kept to prevent any Surprise, by the Enemy attempting to get at the Station that Way.

All these Works inclose 4185 Acres of Ground; whence it is evident here must have been a large Army. You see in several Places where their *Tumuli* or Barrows were, represented by little green Hills.

Having shewn the Fortifications and Out works of the Camp, I will now prove the Part within these on which *Delgovitia* stood.

About half a Mile North-East of *Millington*, on the South Side of a gently sloping Hill, were found several Stone Foundations of Buildings of different Sizes, and of different Shapes; among which were found several Fragments of *Roman Pavements*, *Roman Tiles*, *Flues*, and two *Roman Coins*, all or the chief of which are represented in their respective Colours in TAB. IX. and X. These are all Proofs of the Buildings having *Roman*. There was likewise dug up a Piece of a large Stone Pillar, of about six Feet in Length, but of no regular Order; which, notwithstanding, might yet be *Roman*; for we can't suppose those military
People

People so well skill'd in Architecture as the Artists at *Rome*.

If *Delgovitia* (as *Cambden* hints) be derived from the *British* Word *Delgwe*, which signifies Statues or Heathen Gods, this Place may lay claim to a Title on that Account, much sooner than either *Weighston* or *Godmanham*; for here was dug up a circular Foundation resembling a Temple in all Appearance; being within of 45 Feet Diameter, and the Foundation was near five Feet thick.

Near to this circular Building, but South of it, were the Foundations of two oblong square Building, but with a strait Entrance, not two Feet wide, wherein I apprehend they put in the Fuel and Fire for their Sacrifices; there being evident Marks of burning upon the Stones, they being almost burnt thro': Moreover, in digging in the middle of these two Buildings, we found about half a Yard thick of Ashes, wherein were some few small Pieces of Wood, Fuel, and Pieces of Brute-Bones, chiefly burnt, and a great Part of an Horn of a large Deer, which I now have by me.

East of these were laid open the Foundations of another square Building, wherein we found the Pavements, Coins, &c. as are here represented in TAB. X.

The Situation of these Buildings was very strong, being guarded on the South-East and North-East by a deep Vale, the Sides of which are very steep.

From what has been said, I think there is nothing wanting now to prove this to have been the *Delgovitia*, but to reconcile the Distance as mention'd in the *Itinerary*.

From *Eboracum* to *Derwentio* M. P. VII.
 From *Derwentio* to *Delgovitia* M. P. XIII.

Total M. P. XX.

I had the Road measured from *York* to the circular Building or Temple, and the Particulars were as follows; viz.

	Miles.	Yards.
From <i>York</i> to <i>Stamford-Bridge</i> , or <i>Der-</i>	7	154
<i>ventio</i> , were — — — — }		
From <i>Stamford-Bridge</i> to the first Begin-	7	209
ning of the <i>Roman Works</i> , — — — }		
From the first Barrier to the circular Foun-	3	132
dation, — — — — }		
From the Temple to the East Side of the	2 $\frac{1}{4}$	0
Works is — — — — }		
Total.	17 $\frac{1}{4}$	55

This, tho' not exactly the same Distance mention'd in the *Itinerary*, is yet nearer it than either *Londesburgh*, *Weighton*, or *Godmanham*; for *Londesburgh* is five computed Miles farther than this Place; and if we add 1 Mile and a half more (they in general measuring one Third more than computed at), then it will be about 6 Miles and a half; which, added to 17 Miles and a Quarter, will make *Londesburgh* to be 23 Miles and three Quarters from *York*; which differs more from the *Itinerary* than the Place where I suppose the *Delgovitia* to have been. Both *Godmanham* and *Weighton* are still farther off, the last being 3 computed Miles from *Londesburgh*;

desburgh; and if it measures one Third more, then it will amount to 4 Miles and a half; which, added to 23 Miles and three Quarters, will make the Distance betwixt *Eboracum* and *Weighton*, by *Derventio* and *Londesburgh*, to be 28 Miles one Quarter.

I think this little Variation from the *Itinerary* not to be an Argument strong enough to prove this Place not to have been the *Delgovitia*, when put in Competition with the Situation, &c. which at one View both of the Map and Plan will appear; considering at the same time, that the Proportion betwixt the *Roman Mille Passuum* and our Miles is nearly as 19 to 21.

From what I have said, I think it is evident, that neither *Weighton*, *Godmanham*, nor *Londesburgh*, stand where *Delgovitia* was. I have in the first Place shewn the Probability of this Place near *Millington* being the Station, from the known Prudence of the *Romans*, because one Set of Men could defend the whole four Passes; which could not have been done, had they been placed at *Weighton*, *Godmanham*, or *Londesburgh*.

Secondly, I have shewn, that, from the very Situation and Nature of the Country, there required but little Art to make their Camp, at that time, almost impregnable; the Valleys in general being from 60 to 90 Yards deep, and the Sides thereof very steep.

Thirdly, That from this Camp and *Londesburgh* they might see the whole Country from the *Humber* on the South-East, up the Vale of *York* on the West towards the North-West Side; so that no Army could surprise them that Way.

Fourthly, That they could always have a sufficient Quantity of Provisions, and never want Water, even in the hottest Summers. And,

Fifthly, That there has been a *Roman* Station here, as is evident from the *Roman* Pavement, Coins, Tiles, and Foundations of the Ruins: And if the *Romans* had had a Station at *Weighton*, *Godmanham*, or *Londesburgh*, they would scarce have had one so near the other.

All these Things concur in proving this to be the Site of *Delgovitia*; and there is or can be no Argument brought against it; except that, by the *Itinerary*, the Distance from *Eboracum* by *Devcntio*, is set down at XX *M.P.* and by our Measure the Distance from *York* to the circular Foundation, in the Camp, is only 17 Miles, one Quarter, and 55 Yards; so that there is above two measur'd Miles and one half Difference. In Answer to this, I say, May not the *Itinerary* be as wrong here as in some other Places; which is very evident in several Instances? And as it is wrong in some others, I doubt not but it may be so in this: Besides, the *Romans* might calculate from the Center of *York*; and this Mensuration only goes from the Bar at *Walngate* to the circular Foundation in the *Roman* Camp. But supposing the *Itinerary* to be exactly right, yet, when the Difference betwixt the *Roman Mil. Pass.* and our Miles is calculated, I believe it will end all Disputes on that Score.

*An Appendix to the foregoing Paper, by Mr.
Fr. Drake F. R. S.*

TIME, which subverts and destroys the greatest Works of Mankind, hath an equal Property of bringing Things to Light. The DELGOVITIA of the *Romans* in this Country, so long sought after by *Cambden*, and other Writers, as well as myself, is at length discover'd so far, that there is no need of any more Conjecture about it.

Being informed, in the Year 1745, of some *Roman* Curiosities found in a Field near *Millington*, on the *Wolds*, Dr. *Burton* of *York* and myself set out to survey them. On our coming to the Place, an intelligent Countryman and his Father conducted us to a large plain Field, on the South Side of *Millington* Wood, where we were shewed several Foundations of Buildings under Ground, on the very Stones of which the apparent Marks of Fire may be traced. Two Bases of Pillars, of an irregular Order, and a large Piece of a Column, were also discover'd; several Pieces of tessellated Pavements, *Roman* Bricks, Tiles, &c. were dug up. The Father told us, that, about 40 Years before, he saw the Foundations of a circular Building, about fifteen Yards Diameter, dug up in this Place; which must have been the Vestiges of some *Circus* or Temple: That it had been the Custom for the Inhabitants of their Village, Time out of Mind, to dig for Stones in this Ground when they wanted; and that they must often do, in a Country almost clear of such Materials. The Church of *Millington* itself seems to have been built out of the Ruins of this antient *Roman* Station.

That this was really the DELGOVITIA so long sought after, I think, is beyond Contradiction. The Distance from *York* coincides very justly with the *Itinerary*; nineteen or twenty *Italian* Miles agrees pretty well with our present Computation; and at the same time points out the true military Way from the *Humber* to *York*. Instead of forcing a Road through the Vale, the *Romans* very wisely chose to
mount

mount the Hills as soon as possible ; and therefore directed their *Strajum* from *York* to the Ford, over the River *Derwent* at *Stainfordburg* ; and from thence in a direct Line to *Garrowby Hill* ; which I take to be corrupted from *Barrowby*, many of those *Tumuli* or *Barrows* being near this Place. On the Top of this Mountain, as I may well call it, though the Road turns up it by an easy Ascent, begins a Series of such enormous Works for Fortification, as the like is not to be met with in the whole Island.

This Road on the Summit of the Hill in a strait Line points directly for *Sureby* or *Burlington-Bay*, the *SINUS SALUTARIS* of *Ptolemy*. But another Road to the right takes a different Course, and comes down to the Ruins I have before mentioned. From thence the Road leads directly to *Londesburg*, the Place which I once thought the Station sought for : It passes thro' Lord *Burlington's* Park, where more of it was laid open last Year than I had before seen, in widening the large and noble Canal in that Inclosure. This Place was before a Morass, and the *Romans* were obliged to force a Way through it, which is eight Yards broad, and laid with Stone edgeways to a great Depth. The Road passed up the Hill on the other Side this marshy Place, and divided into two Branches on the Top of it ; one Way pointing through *Weigh-ton* to *Brough* on the *Humber*, and the other by the East End of *Godmondham* directly for *Beverley* ; which now I am convinced also was the *PETVARIA* of *Ptolemy*. From which last Station it must have gone out directly for *Patrington* or *Spurnhead* ; one of which was certainly the *Roman PRAETORIUM*, mention'd as the last Stage in the first itinerary Route of *Antoninus*.

This Sea-Port must be very commodious to touch at, either going or returning from *Gaul*, or the *Belgic* Coasts, and bringing military Stores, &c. from thence, either to *York* or *Malton* ; to which last Place the *CAMOLODVNUM* of *Ptolemy*, another Road branches out, apparently from the Conjunction on the Top of *Garrowby-Hill*, and leads directly to it. But to return to our *DELGOVITIA*.

The

The Situation of this Place is admirable, and the stupendous Works about it, thrown up for a Defence to this Station, and the several grand Roads near it, are not to be described. The Town itself was placed on a Declivity of a Hill, almost full South; and very near its Ruins arise some rapid Springs of excellent Water; and so copious, as, when joined in one Stream, turns a Mill; from which I suppose the Name of *Millington* has proceeded. There was also lately discover'd a Well above a Mile E. *b* S. from these Springs, dug thro' the solid Rock, Twenty-six Yards deep, which must have been a *Roman* Work.

To the South-West there are no Ramparts thrown up; but to the East, North-East, and due North, the whole Country is full of them. The Vales are all of them guarded by small Encampments at their Angles; the Vestiges of the Barracks, now visible, are called by the Country People the *Camps*. These were to prevent any sudden Surprise that Way. On the Hills, from Vale to Vale, some of which are from 60 to 90 Yards deep, and prodigious steep, are thrown up Works, as Ramparts, 12 Yards broad, and proportionably high, which join in right Angles with the Vallies, and serve as a strong Barrier everywhere. Whether they had any Palisadoes upon them is uncertain: but without them they are capable of stopping an Enemy; which they had Cause to be apprehensive of, by its Neighbourhood to the *German Ocean*, from whence the *Saxons* gave them many Alarms.

Dr. *Burton* was at the Expence to have the Whole of these Works measur'd and plann'd out; a Map of which has been exhibited to the *Society* (see TAB. IX.); but, upon my shewing a correct Copy of the Draught to Lord *Burlington*, his Lordship order'd me to get the Survey of the Road leading from thence through his Park at *Londesburgh* to the Division into two added to the former Plan; by which a Course of some

Miles, and many Thousands more Acres on both Sides the Road are given.

F. Drake.

XXIV. *A Letter from Mr. Joseph Ames F.R.S. and Secr. of the Soc. Antiquar. to C. Mortimer M.D. Secr. R. S. concerning a Plica Polonica.*

Good Sir,

Read May 28. 1747. **J**UNE the 22d, 1746, in the Morning, Mrs. *Hannah Coomes*, a neat old Woman, whose Hair (or *Plica Polonica*, as it is call'd) I shew'd the *Society* last *Thursday*, came and gave me the following Informations.

That she was of a gentle Family in *Staffordshire*, who had suffer'd much in the Civil Wars; and that her Mother had her Hair grow in the same manner, whose Maiden Name was *Alice Goldsmith*; but her own Maiden Name was *Hannah Bunby*, born in the Hay-market, in the Parish of *White-Chapel* and baptized at *Aldgate* on a *Saturday* the of *June*, 1645. Her Mother, having such sort of Hair, used to comb hers much to prevent it, till sometimes the Blood came: When she was about 14 Years old she perceived it to grow thick just about the back Part of her Head, and at length grew to this matted long Substance I now saw it, of 109 Inches long. She says

says she has had four Husbands; the first *Nicholas Woodcock*, to whom she was married when about 28 Years old, and had four Children by him; all died young; but observed nothing of their Hair growing so. I am,

S I R,

Your most humble Servant,

J. Ames.

See some Cases of the *Plica Polonica* in these *Transactions*,
No. 417. p. 50. et seq.

C. M.

XXV. *A Description of some Clay Moulds
or Concaves of ancient Roman Coins found
in Shropshire; by Mr. Henry Baker F.R.S.*

Read June 4. 1747. **H**AVING been lately favour'd by a very ingenious Gentleman of *Shrewsbury* with the Sight and Use of some ancient Moulds made of Clay, bearing the same Types and Inscriptions that some of the *Roman* Coins are known to have, and being enabled, by the Assistance of our most worthy *President* of the *Royal Society*, to make out the Inscriptions and Types impressed on the said Moulds, I judg'd it might prove agreeable to this *Society* to see Specimens of so great a Curiosity as these Moulds seem to be, and to have some Account concerning them.

Four of the five in my Possession were found in digging Sand, at a Place called *Ryton* near *Condover* (query whether *Ratunium*), 5 Miles from *Wroxalter* (i. e. *Uriconium*) in *Shropshire*, about a Mile from the *Watlingstreet* Road: These are all of the Size of a *Roman Denarius*, and little more than the Thickness of our Halfpenny. They are made of a smooth Pot, or rather Brick-Clay, that seems to have been well cleansed from Sand or Dirt, and well beat or kneaded, to render it fit for taking a fair Impression. Great Numbers of these were found, but, for want of Care, most of them were broke in Pieces. They are described N°. 1, 2, 3, 4, in the Account below. The fifth, which is twice as thick as any of the rest, was found at *Wroxalter*; the Clay it is made of differs but little from the former; and the Impression on it is also of the Size of a *Denarius*, and of the same Time with the rest.

See TAB. X.

N°. 1. Is probably the Reverse of a *Denarius* of *Severus*, mention'd in *Mezzobarba*, p. 268. from *Noris de Vot. X.* — *Figura velata coram ara sacrificans.* —
VOTA. SVSCEPTA. X.

N°. 2. On one Side — *Caput Juliae Severi.* —
IVLIA AVGVSTA.

On the other Side is the Reverse of a *Denarius* of *Severus*. *Mezzob.* p. 274. — *Victoria gradiens cum fune super scuto.* — P. M. TR. P. VIII. COS. II. P. P.

N°. 3. A Reverse of *Caracalla*. *Mezzob.* p. 286. — *Trophæum de Parthis cum duobus captivis assistentibus.* — PART. MAX. P. ON. TR. P. V. COS.

N°. 4. *Caput Juliae Severi.* — IVLIA AVGVSTA.
N°.

N°. 5. *Caput Juliae Severi.*—IVLIA AVGVSTA.

I remember no Account of any such kind of Moulds being found in other Countries, excepting some said to be found at *Lyons*; but I believe more of them have been discover'd at different Times in *England*; though I cannot pretend to say when, or where. I have been informed, that some Years ago, the Earl of *Winchelsea* had several Impressions or Moulds of this Sort (all joined together Side by Side) on one flat Piece of Clay, as if for the making many Casts at once: They were all of the Emperor *Severus*: And I have seen, in the Earl of *Pembroke's* most valuable Collection a Clay Mould impressed on both Sides, as N°. 2. amongst these also is, one of the Sides bearing the Head of the same Emperor, and the other Side a known Reverse of his. Four of the Five in my Hands are also of *Severus* or his Wife *Julia*, and the other is a Reverse of *Caracalla*, his Son and immediate Successor; so that all, we know of, may be said to be of the same Time very nearly.

They are seemingly intended for the Coinage of Money; though it is very difficult to conceive in what manner they could be employed to that Purpose; especially N°. 2. which has an Impression on both Sides; unless we should suppose they coined two Pieces at the same time, by the Help of three Moulds, of which this was to be the middle one.

If, by disposing these into some sort of Iron Frame or Case (as our *Letter-Founders* do the brass Moulds for casting their Types) the melted Metal could be poured into them, it would certainly be a very easy Method of coining, as such Moulds require little
Time

Time or Expence to make, and therefore might be supplied by new ones, as often as they happen'd to break.

These Moulds seem to have been burnt or baked sufficiently to make them hard, but not so as to render them porous like Bricks whereby they would have lost their smooth and even Surface; which in these is plainly so close, that whatever Metal should be formed in them would have no Appearances like the Sand-Holes, by which counterfeit Coins or Medals are usually detected.

London, May 20.

1747.

XXVI. *An extract of a letter from William Jones Esq; F. R. S. to Martin Folkes Esq; President of the Royal Society; containing a commodious disposition of equations for exhibiting the relations of goniometrical lines.*

T H E O R E M.

Presented July 4. 1747. **I***N a circle whose radius is r, let there be two arcs, A the greater, a the less, each in the first quadrant; put s, t, ſ, and v, for the sine, tangent, secant, and versed sine of an arc; s', t', ſ', the sine, tangent, secant of the complement, and v', the versed sine of the supplement of that arc; let $z = \frac{1}{2}A + a, x = \frac{1}{2}A - a$; or if z and x be put for the arcs, it will be $A = z + x, a = z - x$.*

Then will the terms in any column of the following table, be proportional to their corresponding ones in any other column.

From

*A TABLE of the Relations
of Goniometrical Lines.*

$2s, z$	$\frac{s, 2z}{s, A+a}$	$\frac{v, 2z}{v, A+a}$	$s, A+sa$	$\frac{s, a-s, A}{v, A-v, a}$
$2s, x$	$s, A-s, a$	$\frac{s, a-s, A}{v, A-v, a}$	$\frac{s, 2x}{s, A-a}$	$\frac{v, 2x}{v, A-a}$
$2s, z$	$\frac{v, 2z}{v, A+a}$	$\frac{s, 2z}{s, A+a}$	$s, A+s, a$	$s, A-s, a$
$2s, x$	$s, A+s, a$	$s, A+s, a$	$\frac{v, 2x}{v, A-a}$	$\frac{s, 2x}{s, A-a}$
r	s, z	s, z	s, x	s, x
f, z	r	t, z		
t, z	s, z	$f, z-s, z$		
f, z	t, z	r		
$t, r+t, z$	f, z	f, z		
f, x			r	t, x
t, x			s, x	$f, x-s, x$

From hence, almost an infinite number of theorems may easily be derived; some of which are the following, given here as examples of the use of the table.

$$\text{I. } s, z \times s, x = \frac{1}{2} r \times s, a - s, A = \frac{1}{2} r \times s, z - x - s, z + x = \frac{s, z}{f, x} r r = \frac{s, x}{f, z} r r.$$

$$s, z \times s, x = \frac{1}{2} r \times s, a + s, A = \frac{1}{2} r \times s, z - x + s, z + x = \frac{s, z}{f, x} r r = \frac{s, x}{f, z} r r.$$

II. If A, B, C, be any three angles; $Z = A + B, X = A - B, H = \frac{1}{2} A + \frac{1}{2} B + \frac{1}{2} C$.
Then $\frac{1}{2} r \times v, C - v, X = \frac{1}{2} C + X \times \frac{1}{2} C - X = \frac{1}{2} A + C - B \times \frac{1}{2} B + C - A = \frac{1}{2} H - B \times \frac{1}{2} H - A$.
And $\frac{1}{2} r \times v, Z - v, C = \frac{1}{2} Z + C \times \frac{1}{2} Z - C = \frac{1}{2} A + B + C \times \frac{1}{2} A + B - C = \frac{1}{2} H \times \frac{1}{2} H - C$.

$$\text{III. } \frac{ss, z}{ss, z} = \frac{tt, z}{rr} = \frac{v, 2z}{v, 2z} = \frac{t, z}{t, z}; \text{ Or } \frac{ss, \frac{1}{2} z}{ss, \frac{1}{2} z} = \frac{tt, \frac{1}{2} z}{rr} = \frac{v, z}{v, z} = \frac{t, \frac{1}{2} z}{t, \frac{1}{2} z}$$

IV.

$$\text{IV. } \frac{1}{2}r = \frac{ss, z}{v, z} = \frac{ss, \frac{1}{2}z}{v, z} = \frac{ss, z}{v, 2z} = \frac{s's', \frac{1}{2}z}{v, z}; \text{ and } s, z = \frac{2ss, \frac{1}{2}z}{t, \frac{1}{2}z} = \frac{2s's', \frac{1}{2}z}{t', \frac{1}{2}z}.$$

$$\text{V. } \frac{s, z}{v, z} = \frac{r}{t, \frac{1}{2}z} = \frac{t', \frac{1}{2}z}{r} = \frac{v', z}{s, z}.$$

$$\text{VI. } \frac{t, z}{t, x} = \frac{s, A + s, a}{s, A - s, a} = \frac{t', x}{t', z}; \text{ and } \frac{rr}{t, z \times t, x} = \frac{t', z}{t, x} = \frac{t', x}{t, z} = \frac{s', a + s', A}{s', a - s', A} = \frac{t', z \vee t', x}{rr}.$$

$$\text{VII. } \frac{s, A}{s, a} = \frac{t, z + t, x}{t, z - t, x} = \frac{s, z + \overline{s, x}}{s, z - \overline{s, x}}; \text{ if } z \text{ and } x \text{ are two arcs, then } A = z + x, a = z - x.$$

$$\text{VIII. } s, z + \overline{s, x} = \frac{s, z \times s', x + s', z \times s, x}{r} = \frac{t, z + t, x}{f, z \times f, x}.$$

$$\text{IX. } s', z + \overline{s, x} = \frac{s', z \times s', x + s', z \times s, x}{r} = \frac{rr + t, z \times t, x}{f, z \times f, x}.$$

$$\text{X. } t, z + \overline{s, x} = \frac{t, z + t, x}{rr + t, z \times t, x} rr; \text{ and } t', z + \overline{s, x} = \frac{rr + t, z \times t, x}{t, z + t, x}.$$

$$\text{XI. } f, z + \overline{s, x} = \frac{f, z \times f, x}{rr + t, z \times t, x} r; \text{ and } f', z + \overline{s, x} = \frac{f, z \vee f, x}{t, z + t, x}.$$

XII. In three equidifferent arcs A, z, a ; where $z (= \frac{1}{2} \overline{A + a})$ is the mean arc, and $x (= \frac{1}{2} \overline{A - a})$ their common difference; put $p = \frac{s, x}{r}, q = \frac{s, x}{r}; P = 2p \times s, z, Q = 2q \times s', z$.

Then $s, A = P - s, a = Q + s, a$; And $s, a = P - s, A = s, A - Q$.

XIII. Let $d = v, A - v, a = s', a - s', A$; then $ss, A - ss, a = 2s', A + d \times d = 2s', a - d \times d$.

Note. When an arc is terminated in the second, third, or fourth quadrant, some of the signs (+ and -) of the terms in the preceding theorems, will, by the known rules, become contrary to what they now are.

XIV. Let $A, B, C, \&c.$ be the sines, $a, b, c, \&c.$ the co-sines, $a', b', c', \&c.$ the tangents, of the arcs, $\alpha, \beta, \gamma, \&c.$ whose number is n ; the radius being r ; put S for the product of the n co-sines, $S', S'', S''', \&c.$ for the sum of the products made of every line, every two, three, $\&c.$ sines, by the other $(n-1, n-2, n-3, \&c.)$ co-sines, where the co-line noted by $n-n$ is unity.

$$\text{Then the sine of } \overline{\alpha+\beta+\gamma+\delta, \&c.} = \overline{S-S''+S''-S''', \&c.} \times \frac{r}{r^{n-1}}.$$

$$\text{And the co-sine of } \overline{\alpha+\beta+\gamma+\delta, \&c.} = \overline{S-S''+S''-S''', \&c.} \times \frac{r}{r^{n-1}}.$$

XV. Also putting T' for the sum of the tangents of the arcs, $\alpha, \beta, \gamma, \&c.$ $T'', T''', T''', \&c.$ for the sum of the products of every two, three, four, $\&c.$ tangents; and

$$A = T'$$

$$B = AT'' - T''$$

$$C = BT''' - AT'' + T''$$

$$D = CT'' - BT'' + AT'' - T''$$

$$E = DT'' - CT'' + BT'' - AT'' + T'' \quad \text{Put } R = \frac{r}{r^2}$$

$$\&c.$$

$$\text{Then the tangent of } \overline{\alpha+\beta+\gamma+\delta, \&c.} = A + BR + CR^2 + DR^3 + ER^4 + \&c.$$

XVI. Hence, the sine, tangent, and secant, of any arc a , being represented by s, t, f , the co-sine, co-tangent, and co-secant, by s', t', f' ; those of the arc na are expressed as in the following theorems.

$$\text{Putting } n' = n \cdot \frac{n-1}{2}; \quad n'' = n' \cdot \frac{n-2}{3}; \quad n''' = n'' \cdot \frac{n-3}{4}; \quad n'''' = n''' \cdot \frac{n-4}{5}; \quad \&c.$$

$$\text{Sine of } na = \overline{nA - n''AP + n''BP - n'''CP + n''''DP, \&c.} \times \frac{s^{n-1}}{r^{n-1}};$$

$$\text{where } P = \frac{s'}{r}; \quad A = s; \quad B = AP; \quad C = BP; \quad D = CP; \quad \&c.$$

$$\text{Or } = \overline{nA - \frac{n-1}{2} \cdot \frac{n-2}{3} AP + \frac{n-3}{4} \cdot \frac{n-4}{5} BP - \frac{n-5}{6} \cdot \frac{n-6}{7} CP, \&c.} \times \frac{s^{n-1}}{r^{n-1}};$$

where $A, B, C, \&c.$ stand for the respective preceding terms.

$$\text{Or } = nA + \frac{1+n}{2} \cdot \frac{1-n}{3} A\mathcal{Q} + \frac{3+n}{4} \cdot \frac{3-n}{5} B\mathcal{Q} + \frac{5+n}{6} \cdot \frac{5-n}{7} C\mathcal{Q} + \frac{7+n}{8} \cdot \frac{7-n}{9} D\mathcal{Q}, \&c.$$

$$\text{where } \mathcal{Q} = \frac{s'}{r}; \quad A, B, C, \&c. \text{ stand as before.}$$

XVII. Co-sine of $na = \frac{1 - n^2 P + n^4 P^2 - n^6 P^3 + n^8 P^4}{1 - n^2} \mathcal{E}c. \times \frac{r^n}{r^n - 1}$, where $P = \frac{r^n}{r^n - 1}$

$$\text{Or } r = r + \frac{0 \cdot 1-n}{1} \cdot \frac{0-n}{2} A \mathcal{Q} + \frac{2-1-n}{3} \cdot \frac{2-n}{4} B \mathcal{Q} + \frac{4-1-n}{5} \cdot \frac{4-n}{6} C \mathcal{Q} + \frac{6-1-n}{7} \cdot \frac{6-n}{8} D \mathcal{Q} \mathcal{E}c.$$

where $\mathcal{Q} = \frac{r^n}{r}$; and $A, B, C, \mathcal{E}c.$ stand for the respective preceding terms.

$$\text{Or put } M = \frac{2r^n}{r} \times r; N = \frac{rr}{4r^n}; A = \frac{1}{2}; B = AN; C = BN; D = CN, \mathcal{E}c.; p = n; p' = n-1; p'' = n-2, \mathcal{E}c.$$

And $a' = p; b' = p \cdot p''; c' = p \cdot \frac{1}{2} p' \cdot \frac{1}{2} p''; d' = p \cdot \frac{1}{2} p' \cdot \frac{1}{2} p'' \cdot \frac{1}{2} p'''; e' = p \cdot \frac{1}{2} p' \cdot \frac{1}{2} p'' \cdot \frac{1}{2} p''' \cdot \frac{1}{2} p''''; \mathcal{E}c.$

The co-sine of $na = \frac{A - Ba' + Cb' - Dc' + Ed'}{\mathcal{E}c.} \times M.$

$$\begin{array}{ll} \text{XVIII. Let } A = -n'' + nn' & A = \frac{1}{2} n \cdot n'' - n' \\ B = +n'' - nn'' + An' & B' = \frac{1}{2} n'' A - n'' + n''' \\ C = -n'' + nn'' + Bn' - An''' & C' = \frac{1}{2} n'' B' - n'' A' + \frac{1}{2} n''' - n'' \\ D = +n''' - nn''' + Cn' - Bn''' + An'' & D' = \frac{1}{2} n'' C' - n'' B' + n'' A' - n'''' + n'''' \\ \mathcal{E}c. & \mathcal{E}c. \end{array}$$

The tangent of $na = nt + At^3 r^{-2} + Bt^5 r^{-4} + Ct^7 r^{-6} + Dt^9 r^{-8} \mathcal{E}c.$

$$\begin{array}{ll} \text{Or} & = \frac{n + AN + BN^2 + CN^3 + DN^4 + \mathcal{E}c.}{\mathcal{E}c.} \times t, \text{ where } N = \frac{r^n}{r} \\ \text{Or} & = na' + Ab' + Bc' + Cd' + De', \mathcal{E}c. \text{ where } a' = t; b' = Na'; c' = Nb'; d' = Nc'; \mathcal{E}c. \\ \text{Or} & = \frac{n - n' N + n'' N^2 - n''' N^3 + n'''' N^4, \mathcal{E}c.}{1 - n' N + n'' N^2 - n''' N^3 + n'''' N^4, \mathcal{E}c.} \times t. \end{array}$$

Co-tang^t of $na = r^2 + A't^2 + B't^2 N + C't^2 N^2 + D't^2 N^3 + E't^2 N^4 \mathcal{E}c. \times \frac{r^n}{r^n - 1}$; where $N = \frac{r^n}{r^n - 1}$

$$\begin{array}{ll} \text{Or} & = \frac{1 + A'N + B'N^2 + C'N^3 + D'N^4 + E'N^5, \mathcal{E}c.}{\mathcal{E}c.} \times \frac{r^n}{r^n - 1}; \text{ where } N = \frac{r^n}{r^n - 1} \\ \text{Or} & = \frac{1 - n' N + n'' N^2 - n''' N^3 + n'''' N^4 - n'''' N^5, \mathcal{E}c.}{n - n' N + n'' N^2 - n''' N^3 + n'''' N^4 - n'''' N^5, \mathcal{E}c.} \times \frac{r^n}{r^n - 1}; \text{ where } N = \frac{r^n}{r^n - 1} \end{array}$$

$$\begin{array}{ll} \text{XIX. Let } A = n' & A' = \frac{1}{2} n'' \\ B = An' - n'' & B' = \frac{1}{2} n'' A - n'' \\ C = Bn' - An'' + n''' & C' = \frac{1}{2} n'' B' - n'' A' + n''' \\ D = Cn' - Bn'' + An''' - n'''' & D' = \frac{1}{2} n'' C' - n'' B' + n'' A' - n'''' \\ \mathcal{E}c. & \mathcal{E}c. \end{array}$$

Secant of $na = 1 + AN + BN^2 + CN^3 + DN^4 + EN^5, \mathcal{E}c. \times M.$

$$\text{Or } = \frac{1}{1 - n' N + n'' N^2 - n''' N^3 + n'''' N^4, \mathcal{E}c.} \times M; \text{ where } N = \frac{r^n}{r^n - 1}, M = \frac{r^n}{r^n - 1}$$

Co-secant of $na = 1 + A'N + B'N^2 + C'N^3 + D'N^4 + E'N^5, \mathcal{E}c. \times M$; where $N = \frac{r^n}{r^n - 1}, M = \frac{r^n}{r^n - 1}$

$$\text{Or } = \frac{1}{n - n' N + n'' N^2 - n''' N^3 + n'''' N^4, \mathcal{E}c.} \times M; \text{ where } N = \frac{r^n}{r^n - 1}, M = \frac{r^{n-2}}{r^n - 1}$$

XX. Let c be the chord of an arc (a) of the circumference of a circle, whose diameter is d . Put $N = \frac{c}{d}$.

The chord of $na = nc + \frac{1+n}{2} \cdot \frac{1-n}{3} AN + \frac{3+n}{4} \cdot \frac{3-n}{5} BN + \frac{5+n}{6} \cdot \frac{5-n}{7} CN + \frac{7+n}{8} \cdot \frac{7-n}{9} DN, \&c.$
where $A, B, C, \&c.$ stand for the respective preceding terms.

As the preceding theorems are easily deduced from the first, so the following are most readily seen to be the immediate consequences of these; and all depending upon no other principles than what are generally made use of in common computations.

XXI. Putting $s, s', t, t', f, f',$ for the sine, co-sine, tangent, co-tangent, secant, co-secant, of an arc (a), and v its versed sine; let $q' = \frac{1}{2}; q'' = \frac{1}{3}q'; q''' = \frac{1}{4}q''; q^{iv} = \frac{1}{5}q'''; q^v = \frac{1}{6}q^{iv}; \&c. N = \frac{a}{r}$.

Then $s = 1 - q'N + q'^v N^2 - q'^{iv} N^3 + q'^{viii} N^4 + q'^x N^5, \&c. \times a.$
 $= a - q'^v a^3 r^{-2} + q'^{iv} a^5 r^{-4} - q'^{iv} a^7 r^{-6} + q'^{viii} a^9 r^{-8}, \&c.$
 $= a - \frac{1}{2 \cdot 3} AN + \frac{1}{4 \cdot 5} BN - \frac{1}{6 \cdot 7} CN + \frac{1}{8 \cdot 9} DN, \&c.$ where $A, B, C, \&c.$ stand for the respective preceding terms.

And $s' = r - q'a^2 r^{-1} + q''a^4 r^{-3} - q^v a^6 r^{-5} + q^{viii} a^8 r^{-7}, \&c.$

$= 1 - q'N + q''N^2 - q^v N^3 + q^{viii} N^4 - q^x N^5, \&c. \times r.$
 $= r - \frac{1}{1 \cdot 2} a^2 r^{-1} + \frac{1}{3 \cdot 4} AN - \frac{1}{5 \cdot 6} BN + \frac{1}{7 \cdot 8} CN, \&c. A, B, C, \&c. \text{ as before.}$

XXII. Also $v = q'a^2 r^{-1} - q'^v a^4 r^{-3} + q'^{iv} a^6 r^{-5} - q'^{viii} a^8 r^{-7}, \&c.$

$= \frac{1}{1 \cdot 2} a^2 r^{-1} - \frac{1}{3 \cdot 4} AN - \frac{1}{5 \cdot 6} BN - \frac{1}{7 \cdot 8} CN - \frac{1}{9 \cdot 10} DN, \&c.$

$= \frac{1}{1 \cdot 2} N - \frac{1}{3 \cdot 4} AN - \frac{1}{5 \cdot 6} BN - \frac{1}{7 \cdot 8} CN, \&c. \times r. A, B, C, \&c. \text{ as before.}$

XXIII. Let $A = +q' - q''$ And $A = -A$
 $B = -q''' + q^{iv} + Aq'$ $B' = -B - AA'$
 $C = +q^v - q^{viii} + Bq' - Aq^{viii}$ $C' = -C - BA' - AB'$
 $D = -q^{xi} + q^{xiii} + Cq' - Bq^{viii} + Aq^v$ $D' = -D - CA' - BB' - AC'$
 $\&c. \&c.$

Tangent $t = a + Aa^3 r^{-2} + Ba^5 r^{-4} + Ca^7 r^{-6} + Da^9 r^{-8}, \&c.$

Or $= 1 + AN + BN^2 + CN^3 + DN^4 + EN^5, \&c. \times a.$

Co-tangent $t' = a^{-1} r^2 + Aa + B'a^3 r^{-2} + Ca^5 r^{-4} + D'a^7 r^{-6}, \&c.$

Or $= rr + Aa^2 + B'Na^2 + CN^2 a^2 + D'N^2 a^2, \&c. \times \frac{1}{a}.$

XXIV.

XXIV. Also let $a = +q'$ And $a' = +q''$
 $\beta = -q''' + aq'$ $\beta' = -q'' + a'q''$
 $\gamma = +q'' - aq''' + \beta q'$ $\gamma' = +q'' - a'q'' + \beta'q'$
 $\delta = -q'' + aq'' - \beta q''' + \gamma q'$ $\delta' = -q'' + a'q'' - \beta'q'' + \gamma'q'$
 $\mathcal{E}c.$ $\mathcal{E}c.$

Secant $f = r + a a^2 r^{-1} + \beta a^4 r^{-3} + \gamma a^6 r^{-5} + \delta a^8 r^{-7} \mathcal{E}c.$

Or $= \frac{1 + aN + \beta N^2 + \gamma N^3 + \delta N^4}{r} \mathcal{E}c. \times r.$

Co-secant $f = a^{-1} r^2 + a' a + \beta' a^3 r^{-2} + \gamma' a^5 r^{-4} + \delta' a^7 r^{-6} \mathcal{E}c.$

Or $= \frac{rr + a'aa + \beta'Naa + \gamma'N^2aa + \delta'N^3aa}{r} \mathcal{E}c. \times \frac{1}{r}.$ where $N = \frac{a}{r}$.

XXV. Putting $p' = \frac{1}{2}; p'' = \frac{3}{4}p; p''' = \frac{5}{8}p''; p^v = \frac{7}{8}p'''; p^v = \frac{9}{16}p''; \mathcal{E}c. N = \frac{u}{n}$

Then arc $a = 1 + \frac{1}{3}p'N + \frac{1}{12}p''N^2 + \frac{1}{72}p'''N^3 + \frac{1}{96}p^vN^4 \mathcal{E}c. \times s.$

Or $= s + \frac{1}{3}p'AN + \frac{1}{12}p''BN + \frac{1}{72}p'''CN + \frac{1}{96}p^vDN \mathcal{E}c.$

Or $= s + \frac{1.1}{2.3}AN + \frac{3.3}{4.5}BN + \frac{5.5}{6.7}CN + \frac{7.7}{8.9}DN \mathcal{E}c.$ where $A, B, C, \mathcal{E}c.$

stand for the respective preceding terms.

XXVI. If v is the verfed sine of an arc a , diameter being d , $M = \frac{v}{d}, R = \sqrt{h}$.

Then arc $a = 1 + \frac{1.1}{2.3}M + \frac{3.3}{4.5}AM + \frac{5.5}{6.7}BM + \frac{7.7}{8.9}CM \mathcal{E}c. \times R; A, B, C, \mathcal{E}c.$ are as before.

XXVII. And putting $N = \frac{u}{r}, A = t, B = AN, C = BN, D = CN, \mathcal{E}c.$

Then arc $a = t - \frac{1}{3}AN + \frac{1}{12}BN - \frac{1}{72}CN + \frac{1}{96}DN + \frac{1}{11}EN \mathcal{E}c.$

Or $= 1 - \frac{1}{3}N + \frac{1}{12}N^2 - \frac{1}{72}N^3 + \frac{1}{96}N^4 + \frac{1}{11}N^5 \mathcal{E}c. \times t.$

XXVIII. Also, if c is the chord of an arc (a) ; and $N = \frac{cc}{dd}$

Then arc $a = c + \frac{1.1}{2.3}AN + \frac{3.3}{4.5}BN + \frac{5.5}{6.7}CN + \frac{7.7}{8.9}DN \mathcal{E}c.$ where $A, B, C, \mathcal{E}c.$ stand for the respective preceding terms.

ADVERTISEMENT.

THE Reader is desired to correct the Title of the XIIth Paper of the last Number of these *Transactions* in the following manner :

A Letter from Richard Brocklesby M. D. and F.R.S. to the President, concerning the Indian Poison, sent over by Don Antonio De Ullöa of Seville, F. R. S. and mentioned by M. de la Condamine, of the Royal Academy of Sciences at Paris, in his late Account of the River of the Amazons in South America.

He is also desired in the same Paper, to read towards the Bottom of the first Page, instead of *Mr. Juan Antonio de Loa, Don Antonio de Ulloa*. This Gentleman was one of those sent by the King of Spain to attend and assist the French Astronomers of the Royal Academy of Sciences, in their late Measure of a Degree of Latitude near the Equator. He was taken Prisoner at Cape Breton in his Return home, and brought into England, where his Papers all relating to the Measure of the Degree, and other Astronomical and Philosophical Observations, were by the Favour of the Lords Commissioners of the Admiralty restored to him, to be published in his own Country. An Abstract of the same was however by their Lordship's Leave communicated to the Royal Society, by their President, who was entrusted with the Perusal of the same : And the Author himself, who is a Gentleman of great Merit, was soon after unanimously chosen a Fellow of the Society. Some time after his Return he procured the above-mentioned Specimen of this Indian Poison, which he sent over together, with some Books, as a Present to his Friend the President of the Society.

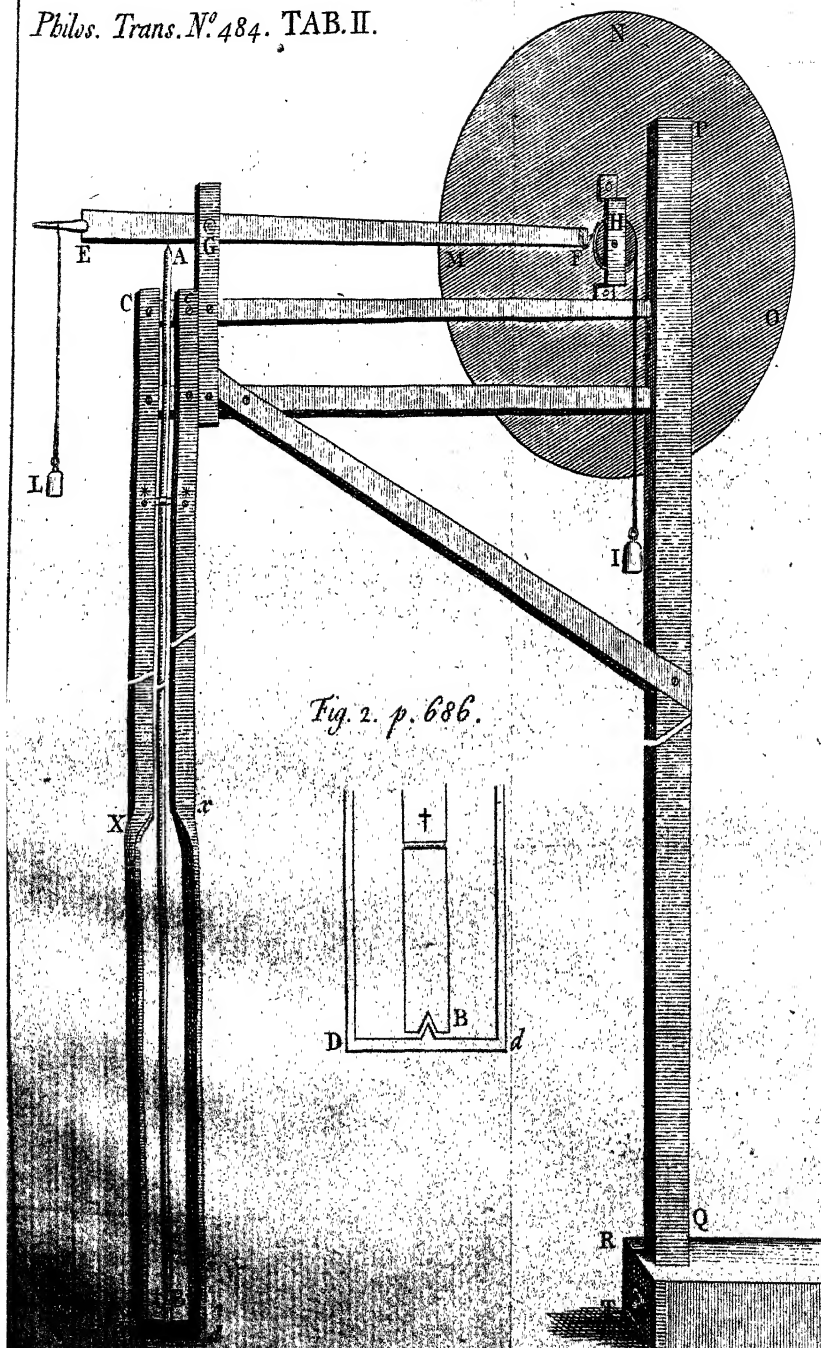
ERRATA.

In *page* 456, *l.* 21. *r.* TAB. II. *Fig.* 8.

In *page* 548, *l.* 28. *add*, by the Direction of the Strokes according to the Rules of Heraldry.

In *page* 549, *l.* 10. *add*, see TAB. IX.

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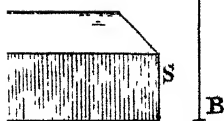
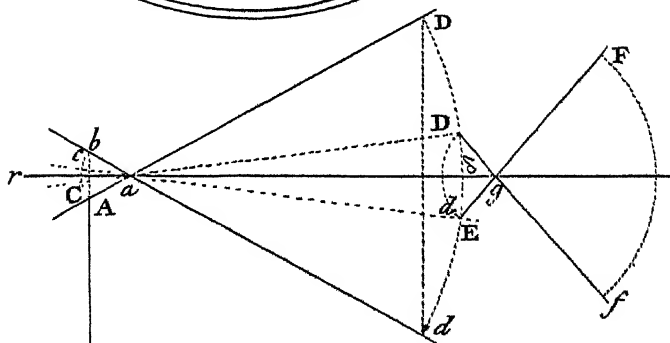
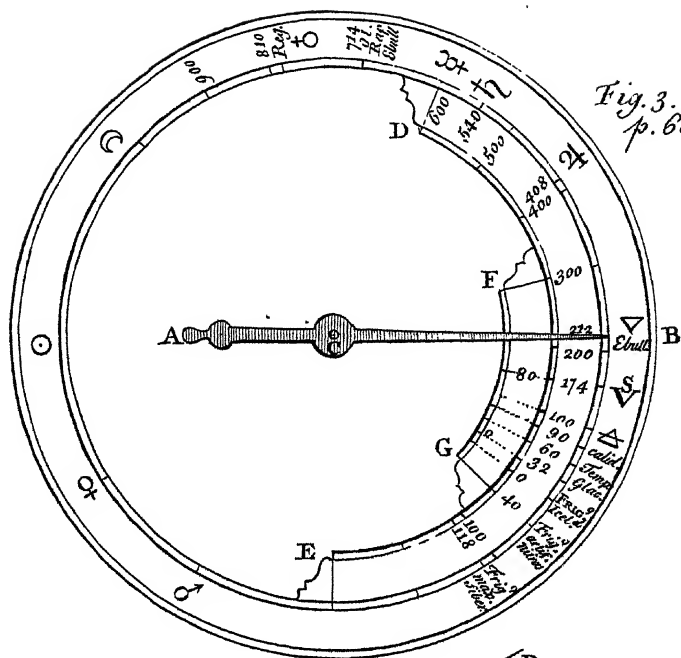


Fig 1 p 575

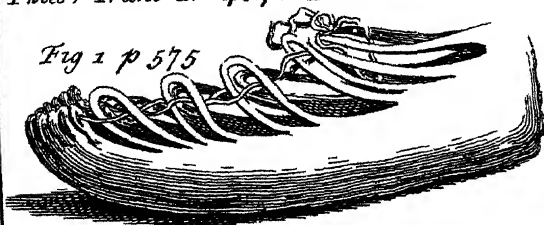


Fig 2. p. 575.

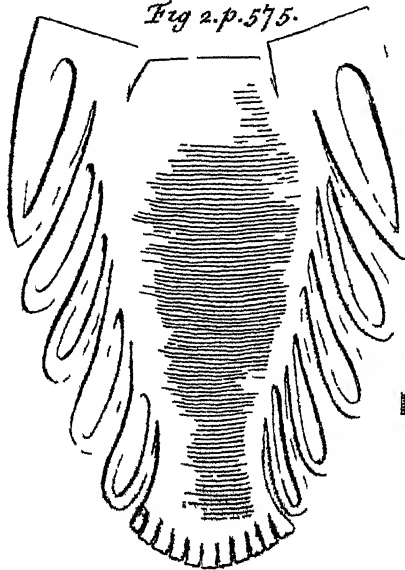


Fig. 4.

*p 630
654*

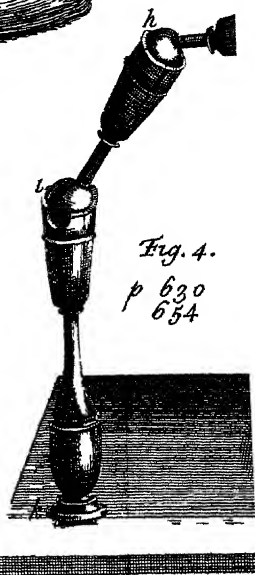
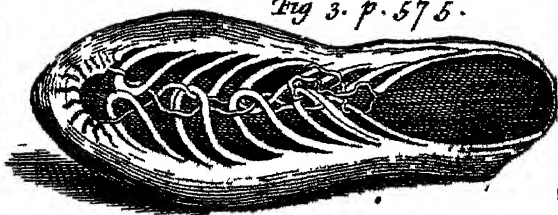


Fig 3. p. 575.



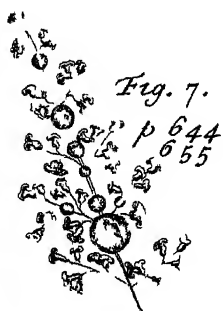
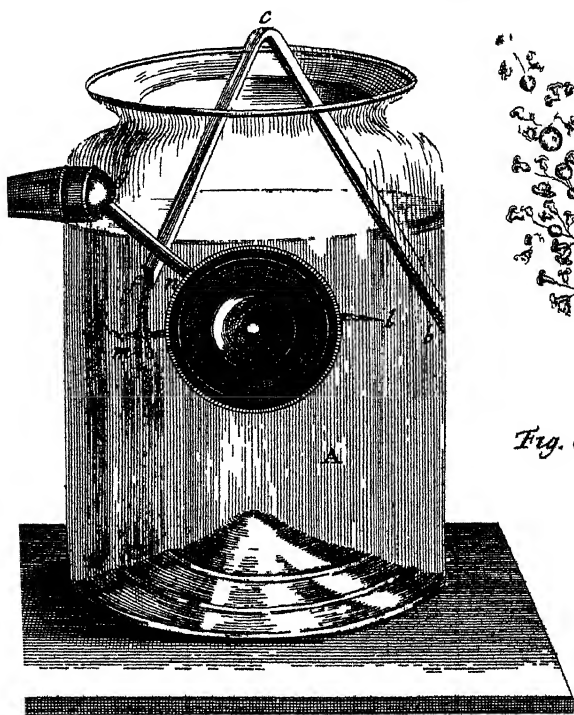


Fig. 7.

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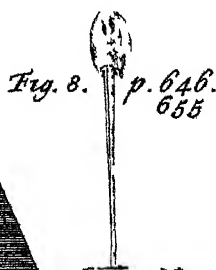


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Fig. 9.
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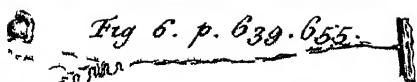


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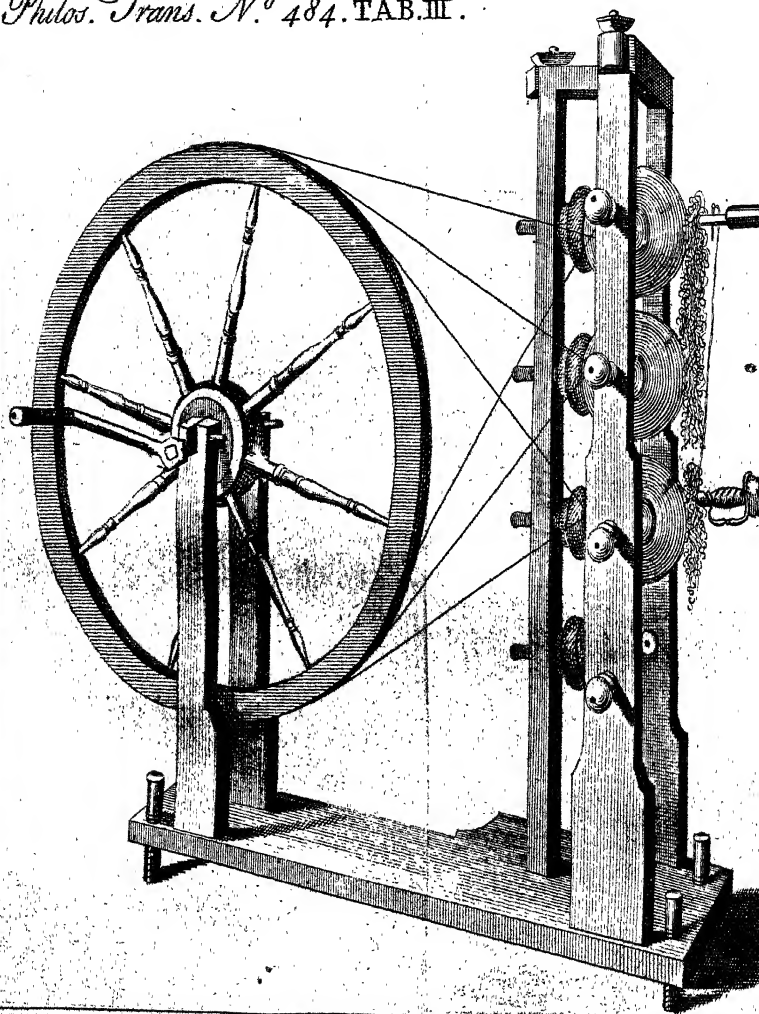


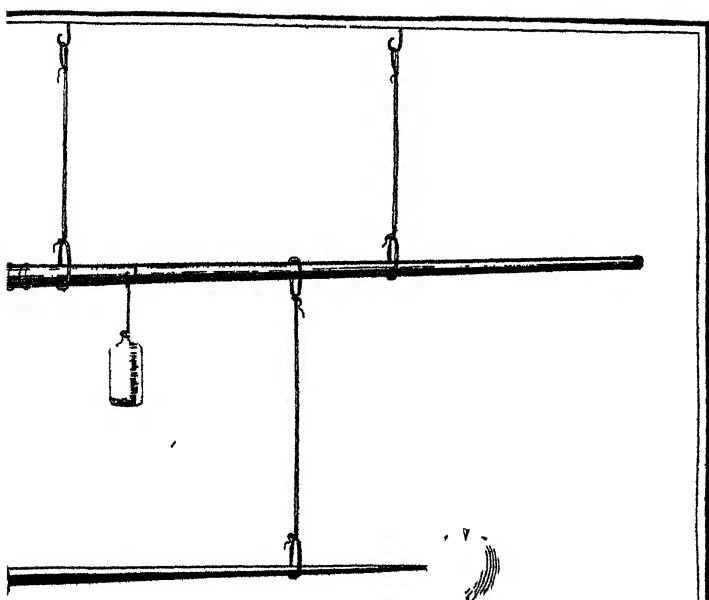
Fig. 5.
p. 655.



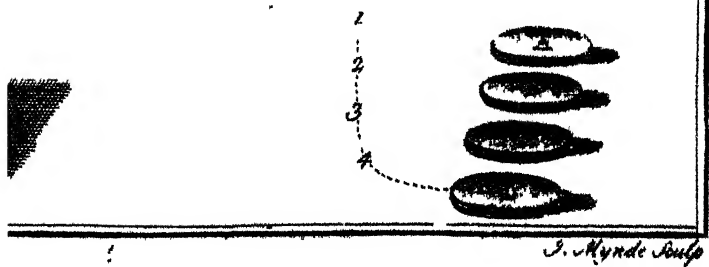
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*Read at a Meeting
of the Royal Society,
October 22. 1747.*

THIS City was over-
thrown and swallow'd
up by an Earthquake
near 1700 Years since.

Some of the most remarkable Curiosities, we saw,
were,

1. A Picture of about five Feet long, and four Feet wide, representing the Education of *Achilles*, by his Master *Chiron* the Centaur. The Figures are about half as big as the Life. That of *Achilles* is standing in a noble Action, and is seen in Front, as the principal Object of the Picture. He seems to hearken with great Attention to, and is looking steadily on the Centaur, who is seen almost in a Side View. The Figures are both finely colour'd, and well drawn; but that of the young Man most exquisitely so.

2. Next to this is a Picture of about three Feet and an half high, and narrow, in which is a Woman sacrificing. The Figure is about two Feet high. This Picture seems to have been taken out of some Compartment of Ornaments.

E e e

3. Next

3. Next to this is a broken Piece, representing the Judgment of *Paris*. The Figures are about the same Size as that last above-mention'd. They are not intire; the bottom Part being broken off about the Knees. This is also a very fine Picture; but it is impossible to judge of all its Beauties, as it is extremely changed and decayed; which is quite contrary to all the others, but, in particular, to that of *Chiron* and *Achilles*; which is in a manner as fresh as if it had been painted but Yesterday.

4. The next is a fine Picture of the Story of *Virginia*. The Figures are something bigger than those in the abovementioned. The Characters and Expressions of the Heads are admirable. That of *Appius* gives a just Idea of the furious Transports in which the Artist designed to describe him. *Virginia* is weeping; and, in a word, all the Figures are finely disposed, and the Characters well adapted to the Subject.

5. Two large Pictures were in a Nich in a *Basilica*, about five or six Feet high. The first represents *Theseus* victorious over the *Minotaur*. He is standing in a free and fine Posture: One Foot is on the Head of the *Minotaur*. But what seemed odd to me, was the Figure of that Monster itself, which I had always seen differently represented; for, in this Picture, the Head only represents that of a Bull, which is joined to the Body of a Man. Several little *Genii*, or *Cupids* (as we call them), all seem impatient to shew their Respect to their Deliverer: One kisses his Hand, another clasps round his Leg, and several others are in different Attitudes of Gratitude.

titude. The Figures are almost as large as small Life.

6. The other Picture represents *Hercules* and the Goddess of Nature. The Figure of *Hercules* is standing (seen in a Side View) reposing on his Club; something like the Statue in the *Farnese* Palace at *Rome*. There is a Victory crowning the Hero, and the Goddess is sitting before him, and seems to applaud and thank him for his Labours. There are Numbers of symbolical Figures besides in this Picture. Behind the Goddess is a Satyr, and at *Hercules's* Feet a Boy sucking a Doe. My Friend, who conducted me, took particular Notice, how delicately the Doe seemed to dispose of her Legs, not to hurt the Child; whilst at the same time she is licking his Knees, as a Mark of her Tenderness for him. This Picture is equal to the first-mentioned; being exquisitely finely drawn and coloured, and well composed.

7. There is a little Picture, which I thought extremely odd for its Composition. It is about 1 Foot and an half long, and eight or nine Inches high: It is a Parrot drawing a Chariot something like our modern Chaises. In the Chariot sits a sort of large Horse-fly, whose two Horns serve for the Bridle and Reins to guide the Parrot.

8. Two Pictures, of about four Feet and an half long, represent the Stage of a Theatre, with Comedians playing their Parts upon it. The Perspective in these Pictures is very well observed.

9. A Wedding, consisting of three Figures only. They are much in the same Taste of those of *Aldobrandini's* Marriage at *Rome*. There are besides

Numbers of little Frizes representing Sacrifices, and other Ceremonies, of the ancient Pagans; most of them on black or red Grounds.

These Pictures shew, that the Antients understood Perspective and Landſchape, I mean, the Keeping particularly, which I have heard ſtrongly diſputed; but no one that has ſeen theſe Pictures will, I believe, make any Doubt of it.

It would be impoſſible for me to give you an exact Deſcription of all the Pictures; as there are ſo many entire, beſides the Bits, and Fragments of others. Of ſome, the Heads only remain; and of others, Picces of Figures; Numbers of ſmall Landſcapes; Views of Architecture; Flowers and Fruit, painted extremely light and elegantly. There are even ſome groteſque Pictures, ſomething in the Taſte of *India* Painting. Moſt of the ſmall ones have been taken out of Compartiments: The Guardian ſhewed me ſeveral Places from whence they had been taken. They ſtill preſerve a Beauty ſuperior to any thing we ſee now-a-days: The Colouring, Drawing, and Liberty of Pencil, may vie with the Works of any Maſter, even of *Raphael* himſelf.

There are two Rooms full of them. — Perhaps you will ſay I have given you an Account but of very few. — In the firſt place I muſt inform you, that no one is admitted without an Order from the King's Superintendant; and that, when one does ſee them, the Guardian has Orders not to permit any Perſon to take any Sketch or Account whatever of the Pictures. My Friend went thither but
once,

once, and that after Dinner, when we came down from Mount *Vesuvius*. The Account I have herein given is all I could particularly remember in the Evening when we came home. They are continually finding more Pictures every Day; and I do assure you, that had I a Month to spare, I would willingly go on Foot to *Naples*, to have the Pleasure of studying those I have already seen, and seeing those which have been discovered since. *

N. B. Cardinal *Albani*, at *Rome*, has an antique Group of *Theseus* and the *Minotaur*; where the *Minotaur* has the Head only of a Bull, as in the Picture above-mentioned.

II. *A Letter from Mr. G. Stovin to his Son, concerning the Body of a Woman, and an antique Shoe, found in a Morass in the Isle of Axholm in Lincolnshire.*

Read Oct. 22. 1747. **T**HE Beginning of June last, a labouring Man, of *Amcotts* in the Isle of *Axholm*, in the County of *Lincoln*, was digging Turf or Peat in the Moors of *Amcotts*; and, at about six Foot from the Surface, his Spade cut the Toe of a Sandal, which dropped into the Pit he was graveing Peat in; also Part of the Foot dropp'd in, which terrified the Man, and he left it. Hearing of this Discovery, I went and took some Servants with me, to make further Discovery; when we soon found the other Sandal (which I now send you

* See more of these Curiosities in these *Transf.* N°. 456 and 458.

you whole and firm). It was very soft and pliable, and of a tawny Colour, with all the Bones of that Foot in it, and all the grizzly Part of the Heel: And proceeding further, we found the Skin and Thigh-Bones, which I measured to be eighteen Inches long. We then found all the Skin of the lower Parts of the Body, which was of the same Colour of the Sandals, and very soft, with fresh Hair upon it, &c. which distinguish'd it to be a Woman, The Skin drew or stretch'd like a Piece of Doe-Leather, and was as strong. We then found the Skin of the Arms, which was like the Top of a Muff or Glove, when the Bones were shaken out. We then found this Hand I have sent, with the Nails as fresh as any Person's living; which are now, both Hand and Nails, shrunk very much, since it was expos'd to the Air: This Hand is the Lady's natural Skin so tann'd, with the Nails. We left the Bones in the Fingers, where the Nails are, for fear the Nails should drop off, if that Joint was taken out.

I want to be informed what Age they wore those Sandals in. These must, I think, be very antient, and have most certainly been made of a raw Hide, by reason they, and the Skin of the Lady, were both of one Colour, and both had one Tanner; which I presume, is the Moor-Water; which is exactly of the Colour of Coffee; and made so by reason of such great Quantities of Oak and Fir-Wood, that we frequently dig out of these Moors; several Oak-Trees affording 1000 Pales for fencing, five Feet and an half long, and six to eight Inches broad; which Oak-Wood is rated as black as Jet. The Fir-Wood retains its Turpentine-Smell, and in hot Weather
(when

(when it is exposed to the Sun) the Turpentine will drop from it. This Wood is frequently riven into Laths for the Roofs of Houses or Floors; and what is remarkable, no Worm will touch them. The Pales mentioned above are sold from 10 to 15 Shillings *per* hundred. We frequently find Hazle-nuts and Fir-Apples in Abundance; which I think is a plain Proof, that the Trees fell in Autumn, when the Fruits were at Maturity. I think Dr. *Shuckford* makes it plainly appear that the General Flood happen'd in Autumn.

This Lady in all Probability was overwhelmed by some strong Eddy of Water; for she lay upon one Side bended, with her Head and Feet almost together.

It appears by the Maps of the Country, that this has been the Rendezvous of all the Waters from the South, West, and North Parts of the Kingdom; as for Instance, the River *Dun*, from *Doncaster*, *Rotherham*, and *Sheffield*, which took in many more Streams; as the *Idle*, *Trent*, *Torn*, *Dare*, *Rother*, &c. &c. Then the River *Trent*, which runs South to *Gainsbrough*; then to *Torksey*, *Newark*, *Nottingham*, *Derby*, *Burton* upon *Trent*, *Strafford*, *Trent-ham* in *Staffordshire*: And takes in a vast Number of Rivulets: Then the *Ouse*, which comes from near *Richmond*, and takes in the *Ure*, *Wharf*, *Bishop's Dike*, *Aire*, *Calder*, and a great Number of Rivulets; which are all lost in that famous Estuary the *Humber*.

It is also to be observed, that here is one Morass twenty Miles round, Part in *Hatfield-Chace*; another ten Miles round in the same Chace, where the

the famous *William* of *Lindham* had his Cell. In the middle of it, where his Body was found, for eight Miles round, is all a *Morals*.

The *Connoisseurs* will give you their Opinion in the chief thing I want to know, which is, in what Age those Sandals were worn, and by what Nation : For it is not like the *Scots* or *Irish* Broges; though the *Scots*, I think, formerly inhabited but a little Way off, *to wit*, North of *Humber*. Perhaps the *Danes* may wear such, or the antient *Saxons*; for both these People must be well acquainted with these Parts, as the *Danes* under *Edgar-Atheling* incamped a whole Winter in this Neighbourhood, and had a Station at *Gigansburgh*, now *Gainsbrough*, upon this River *Trent*.

At *Boxby* was a famous *Roman* Pavement, 15 Yards square, the *Roman* Road, &c.; also a square Platform at *Aldbrough*, which I take to be *Roman*, though no Discoveries have as yet been made there; but at *Roxby* large Quantities of *Roman* Coins have been found. I am

Your ever affectionate Father,

G. Stovin.

P. S. As to this Water upon these Moors preserving human Bodies *, it is most certain; *viz.* Part of a Body taken up at *Geel* by your Grandfather Mr. *Empson* 50 or 60 Years ago, and one in the great Moor near *Thorn*, about 7 Years ago, with the Skin like tann'd Leather, the Hair,

* See these *Transf.* No. 434. p. 413.

Hair, Teeth, and Nails quite fresh. — You will see the Sandal * is of one Piece of Leather, and a Seam at the Heel, with a Thong of the same Leather. See TAB. I. *Fig. 2.* and *3.*

It is the Skin of the Hand that is stuff'd, which has suffer'd by the Spade. †

III.

* Mr. *Gatesby* F. R. S. Author of the History of *Carolina*, &c. being present, said, this Shoe or Sandal was exactly like what the *Indians* in *Virginia* wear at this Day, and call *Mokassin*.

† That ingenious Artift and skilful Antiquary Mr. *Geo. Vertue* communicated to me (*G. M.*) his Sentiments concerning this Sandal in the following Words :

‘ When the above Letter was read at the *Society of Antiquaries*, there was produced a Hand of the Woman therein mentioned, and a Sandal or Shoe taken from one of her Feet ; it being made of Leather, tann'd Ox-Hyde ; but remarkable for being cut out of one flat Piece, (see *Fig. 3.*) so as to fold about the Foot and Heel ; the Form and make being so contriv'd without under Heel-Piece, as to be flat to tread on ; the Shape, that of a Woman's Foot, and the Toe round-pointed. This being of an antient Form, the Society order'd an exact Draught to be taken of both that and the Hand ; which Drawings are preserved amongst others belonging to that Society. It may be observed concerning the Antiquity and Use of Leather Shoes in *England*, that this Shoe or Sandal appears by its Form to be ancient. I conceive it was before *Edward the IV.*'s Time, when, by Custom, piked Shoes had increased in Length, that all such who wore them in excessive Length were to be mulcted, or have them cut shorter, in passing in or out of the City-Gates of *London*. This very likely had passed amongst the better sort of People about the Kingdom ; for *Chaucer* in his Time mentions the Use of long piked Shoes, so long as to be tied up by Strings or small Chains to their Knees.

‘ Thus it might have been with Mens Shoes, but not in so long a Degree for Womens Use ; tho' observing antient Pictures of Men and Women in Books of Illuminations, piked Shoes appear in several Reigns from *Ed. III.* to *Rich. III.* in *England*.

F f f f

‘ Also

III. *A Letter from Mr. Henry Baker F.R.S. to the President, concerning the Grubbs destroying the Grass in Norfolk.*

S I R,

Read Nov. 5. 1747. **H**AVING seen some Letters lately sent from the Counties of *Norfolk* and *Suffolk*, giving an Account, that prodigious Numbers of what one Letter calls *Grubs*, and another large *Maggots*, full as thick and almost as long as a Man's little Finger, are dispersed over the Fields, and do abundance of Mischief in those Counties, I immediately imagined (tho' the Accounts were very imperfect, being sent by People wholly ignorant of Natural History), that they must be the *Aurelia* or *Chrysalides* of some Species of Beetle: And desiring to get what farther Information I could concerning them, I wrote with that Intent to my ingenious Friend Mr. *Arderon* at *Norwich*, F. R. S. whose Answer (with some Additions of my own) I shall

‘ Also on our antient Monuments of Stone or Alabaſter cumbent Statues have moſtly piked Shoes. But ſome of earlier Date than *Ed. III.* have broad turn-up Shoes at the Toes, of the ſame like Form and Make as this *Womans*. The *Mens* broad Toes, and the *Womens* narrow,

‘ Therefore I conclude this very Sandal could not well be earlier than *Ed. I.* or *Hen. III.*; alſo, that the cutting the Form, and ſowing to form the Heel cleverly, by a ſitching behind the Heel with a ſmall Leather Thong, may have been in Uſe before that of waxed Thread uſed by Shoe-makers, formerly called Cord-wainers.’

Fig. 1. ſhews the Shoe ſide-ways, laced, as when upon the Foot.

Fig. 2. the ſame ſeen from above.

Fig. 3. the ſame unlaced, and laid flat, to ſhew the manner of its being cut out of the raw Hide.

shall beg Leave to lay before you, after first intreating your Patience to read a Description of these Insects, published in the *London Evening Post* of *October* the 29th, as it is the same in Substance with the private Letters that put me on enquiring after them.

Extract of a Letter from Norfolk, to a Friend in London.

I Forgot to tell you in my last of the Grubs that are in many Parts of our Country. They attack the Corn-Fields sometimes, and spoil all the Crops, but haunt chiefly the richest Meadows, where they work between the Turf and the Soil, eating the Roots of the Grass to that degree, that the Turf rises and rolls up, with almost as much Ease as if it was cut with a Turfing-Spade; and underneath the Soil is turn'd to a soft Mould, like a Bed in a Garden, for about an Inch deep; in which lie the Grubs, in a curved Posture, upon their Backs, all and every one of them with only the Tips of their two Ends in Sight, the rest of their Bodies buried in the Mould. They are in general about an Inch and an half long, and as big as the Stem of a Tobacco Pipe, near the Bowl; they have red Heads, white shining Bodies, a little hairy on the Back, and the Rump End is ready to burst with a dirty looking Stuff, easily seen through the transparent Skin; they have six hairy Legs, three on each Side, all near the Head, two *Forceps*, or Jaws, like a Hornet, with which they cut asunder the Roots of the Grass, and destroy whole Meadows, with-

' out any Remedy yet found out to destroy them;
 ' they have no Eyes, as can be seen. Whether they
 ' are in their ultimate State, or are to be Flies, I
 ' know not, for we could find no *Aurelia*. The
 ' first News we heard of them was about two Years
 ' ago, by a Gentleman who lives near *Norwich*,
 ' and then were supposed to be new Comers. This
 ' Summer they have been much in *High Suffolk*,
 ' to the Farmers and Graziers great Loss; and we
 ' now hear they are likewise in some Part of *Essex*;
 ' they are often discover'd by Hogs, who, as I am
 ' inform'd, are greedy of them at first, but having
 ' once had their Bellies full, never care for them
 ' after.'

Of these Grubs Mr. *Arderon* gives the following
 Information. — They are, says he, a Species of In-
 sects but too common about *Norwich*, and, to my
 own Knowledge, have been more or less numerous in
 this County for these twenty Years past. They are
 the *Eruca* of the *Scarabæus arboreus vulgaris major*
 of Mr. *Ray*, that is the Tree-Beetle, or blind Beetle,
 vulgarly in *Norfolk* called the *Dor*.

In different Parts of *England* they are called the
 Brown Tree Beetle, the Blind Beetle, the Chafer, the
 Cock-Chafer, the Jack-Horner, the Jeffry-Cock, the
 May-Bug, and the Dor. By the *Dutch* they are named
Baum-kaefer, *Roub-kaefer*, *Koren Worm*, or Corn-
 Worm, because they destroy the Roots of Corn; and in
Zealand, *Molenaers* or *Millers*, as *Goedartius* says,
Chap. 78. because they bite the Leaves of several
 Sorts of Trees into Particles as small as if they were
 ground. In *England* I have likewise heard them
 called

called *Millers*; but supposed it to be from a white mealy Powder wherewith their Wings are covered. The *French* call them *Hanetons*.

This Insect has two Pair of Wings, one filmy, and the other scaly: The first Pair fold together under the latter, and remain quite hid, unless when spread out for Flight. The *Elytra* or case Wings are of a reddish light-brown Colour, and seem sprinkled with a white Powder that may easily be wiped off; the Legs and pointed Tail are whitish, the rest of the Body brown, except at each Joint on the Sides of the Belly, where there is an indented Line of white. The Circles round the Eyes are yellowish, and so are the *Antennæ*, which are short, and terminated by lamellated spreading Tufts, capable of being opened more or less.

It is probable the Females make Holes in the Ground with their sharp Tails, and there deposit their Young: But whether at first they are small *Erucae*, or Eggs from whence such *Erucae* are hatched, I don't pretend to say: 'Tis certain however, that these *Erucae* are extremely mischievous, by devouring the Roots of almost every thing where they come, and in some Grounds they are to be found in such Numbers as is scarcely credible.

I have seen, says Mr. *Arderon*, whole Closes of fine flourishing Grass, in Summer-time, become wither'd, dry, and as brittle as Hay in a few Weeks, by this Vermin's eating off the Roots; in doing which they are so dextrous, that many Yards of this wither'd Grass might be rolled up in one Piece, all the Fibres that fasten'd it to the Ground being gnaw'd away.

Closes

Cloves of Turnips often undergo the same Fate from these devouring Insects, which one would think designing to do as much Mischief as possible (could we suppose them capable of any Design); for when one of them fixes upon a Turnip, he eats only the middle small Root, which soon causes it to wither and die, and then moves on to the next. In like manner they destroy the Roots of Wheat, Rye, &c. and almost every other useful Vegetable that happens in their Way. And what makes this Pest the more deplorable, is the long Time of their Continuance in their *Eruca*, or most mischievous State, which, according to *Goedartius*, is four Years at least: But *Mouffet* writes, that in *Normandy* they are observed to be most numerous every third Year, which is therefore called *L'An des Hannetons*. And it is not improbable, that in the open Fields where they are well fed, they may come to their perfect State a Year sooner than those did which *Goedartius* almost starved in glass Jars.

Mr. *Arderon* says, he has frequently been told by People of Credit and Observation, that neither the severest Frosts of our Climate, nor the being immersed in Water, will destroy these *Eruca*; some having been exposed for many Days to the keenest Frosts, and others cover'd with Water for as long a Time, which notwithstanding were found to revive, and become as vigorous as ever.

Crows and Hogs devour these *Eruca* greedily; but their Numbers are too great to be much diminished thereby. The most effectual Way, tho' very laborious, is to beat them off the Trees in the Day-time with long Poles, and then sweep them together

ther and burn them. On a Farm at *Heathal*, near 5 Miles S. W. from *Norwich*, of 80 *l. per Annum*, belonging to St. *Helen's* Hospital in this City, in the Occupation of Mr. *James Ebdin*, these Insects were so numerous last Year, that the Farmer and his Servants affirmed they gather'd eighty Bushels of them, the *Eruca* of which had so spoiled the Produce of his Farm, that the Court of this City, in Compassion of the poor Man's Misfortune, allowed him 25 *l.* The Order for which I send you a Copy of, dated *Dec. 5. 1746.*

In the Day-time few of the Beetles fly about, but conceal themselves under the Leaves of Oaks, Sycamores, Limes, &c. where they seem asleep till near Sun-set, when they take Wing and fly about the Hedges, as thick as Swarms of Bees; at which time they frequently dash themselves against People's Faces with great Violence, and by their so doing occasioned the common Proverb, *As blind as a Beetle.*

Mouffet tells us, it is recorded, that on the 24th of *February*, in the Year 1574. there fell such a Multitude of these Insects into the River *Severn*, that they stopp'd and clogg'd the Wheels of the Water-Mills: As to which I must take notice, that their coming so early in the Year was no less extraordinary than their Multitudes; for the larger Species seldom appear till the Month of *May*; and a smaller Sort, which come out in *July* and *August*, are seldom seen after the Evenings grow cold.

We are told in the *Transactions* of the *Dublin* Society, that the Country People in one Part of that Kingdom suffer'd so greatly by the Devastation made
by

by these Insects, that they set Fire to a Wood some Miles in Length, which parted two adjacent Counties, to prevent their dispersing themselves any farther that Way.

This, Sir, is all I shall trouble you with at present concerning the Grubs mentioned in the News-Paper, as well as in private Letters, without distinguishing of what Kind they are; and if this Information may prove acceptable, it will be a singular Pleasure to him who begs the Honour to be esteemed,

Strand, Nov. 5.

S I R,

1747.

Your most obedient,
and most humble Servant,

H. Baker.

Mr. *Arderon*, in a subsequent Letter to Mr. *Baker*, writes, that Mr. *Ebdin* solemnly declared, before the Committee of St. *Helen's* Hospital, that the Damage done to him that Year amounted to 100*l.* and upwards. Three Gentlemen, appointed to inspect into the Truth of his Complaint, visited the Farm in the Harvest-Time, when, amongst other things, they found those Ears of Wheat, which had Part of their Roots eaten off, to look pale and wan, and when rubb'd, to afford nothing but small wither'd Corn, not fit for any Use, and the Straw came up with the least Touch; and that these *Eruca's* do most Damage, where the Soil is richest.

Tho' many burn these Flies, when they have beat them off the Trees, Mr. *Ebdin's* Method was to spread Cloths under the Trees, where he saw them hanging thickest, and when beaten off, to wrap them up in the Cloths, and beat them to Pieces with wooden Beetles.

Mr. *Arderon* says, he had the above Remarks from Mr. *Richard Humphrey* one of the Committee who inspected the Farm; and he adds that some ingenious Persons account for the large Increase of these Insects from the Decrease of Rooks in this County, which they say greedily devour them in their Grub-State; and the Decrease of Rooks is owing to most of the ancient Rookeries having been destroyed by the large Fall of Timber-Trees made of late Years, which has obliged the Rooks to remove into other Parts of the Kingdom.

Nov. 21. Mr. *Arderon* dug up with his Cane two of these *Eruca's* within a Foot of one another in the *Upper Close*, near the middle of the City of *Norwich*, surrounded with Houses; they were about half-grown, an Inch and half long, and two Teaths in Diameter.

IV. *A Letter from J. Wall M. D. to Edward Wilmot M. D. F. R. S. and Physician to His Majesty, concerning the Use of the Peruvian Bark in the Small Pox.*

Read Nov. 5. ^{1747.} THERE is not perhaps any Disease more fatal than the Small-Pox, when attended with Hæmorrhages, purple Spots, and other terrible Symptoms. I think one might venture to affirm, that, in proportion to the Number of the Sick, fewer recover under these Circumstances, when treated by the common Methods, than do even from the Plague itself. A Remedy therefore in any Degree adequate to the Malignancy of the Disease, which could restore the broken Crasis of the Blood, and correct the putrid gangrenous Disposition of the Juices, must be of infinite Service to Mankind; and such a Remedy the *Bark* appears to be.

The first Author I meet with, who mentions the Use of the *Bark* in any Stage of the Small-Pox, is Dr. *Morton*; and he recommends it only in the (a) Decline of the Disease, when the secondary Fever
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(a) Ubi viribus veneni, durante exanthematum eruptione, ptyalismo vel quovis alio modo ex parte fractis, licet non penitus deletis, atque inde usque ad stadium declinationis intus cohibitis et revirescentibus, febris recidiva indolem præbuerit benignam, periodicis exacerbationibus & remissionibus sese alternatim excipientibus (quem typum in variolis mediis, prope ad malignas accedentibus, fere semper observare licet) curatio nulla methodo
G g g aut

is mild, and puts on the Type of an Intermittent. But, as the *Prima Viæ* are then very much loaded, even in the mildest Sorts of the confluent Small-Pox, this Remedy (if used only in that Stage) does not seem to promise much; at least, not till the *Saburra* in the first Passages be in some degree carried off. And accordingly, the learned and judicious (b) Dr. *Frewin* found that it did not at all answer, even under his Direction, till the first Passages had been emptied.

Since that time, the good Effects of the *Bark* in Mortifications and putrid Ulcers have been discover'd; and Dr. *Monro*, reasoning from thence, did some time ago (c) recommend it in the first Stages of the Small-Pox; as promoting the Maturation, and procuring a mild well-condition'd *Pus*. On his Authority alone, had I no other Reasons, I should have been induced to try this Medicine in some bad kinds of that Distemper, where the Suppuration does not proceed well; but having also seen the *Bark* of great Service in other Diseases, where the Texture of the Blood seem'd much broken; and particularly in Petechial and Purple Fevers, attended with Hæmorrhages,

aut remedio certius ac felicius absolvitur, quam celebri antidoto, cortice sc. *Peruvianæ*, qui reliquias venent subigendo, ut in cæteris quibuscunque febrilem

æuum

subsequente reconvalescit, &c. &c. *Morton Pyralog. De Variolis,*

(b) In his Letter to the late learned Dr. Friend. *Comment. de Febribus,*

(c) *Med. Essays, Edinburgh, Vol. 30. Art. 10.*

rhages, and other terrible Symptoms; I was the more ready to conclude that it might be of great Use in the Small-Pox under the like Circumstances; more especially, as Dr. *Monro*, in the Paper above-mention'd, assures us, that, by its Use, ' *Petechiæ*, in ' several variolous Patients, became gradually more ' pale-colour'd, and at last disappear'd.' I was the less fearful of making an Experiment which was new (at least in these Parts) in a Distemper attended with such fatal Symptoms, because scarce any recover under them by the common Methods. For, to use the late celebrated Dr. *Fremd's* Words [*Epist. de Purgant.*], ' *Ad has angustias cum ventum sit,* ' *nequaquam oportet dubitare, an anceps præstet dare* ' *remedium, an nullum: quod, utcumque anceps, po-* ' *test fortasse vitam servare, potest saltem ad dies* ' *aliquot protrahere. Sed quid si non successerit?* ' *satis est, ut quis hoc Medicinæ genus jure expe-* ' *riri putetur, si id vel in millefimo corpore profe-* ' *cisse compererit.*'

The first Person to whom I gave the *Bark* in the Small-Pox, was one Mr. *Hall*, a young Gentleman of about 24 Years of Age, who lived within two Miles of this Town. After heating himself violently at a Ball, he was seiz'd with the previous Symptoms of the Small-Pox. The third Day of his Illness he sent for me; and before that time he had been blooded, had taken a few Salts and *Manna*, which had given him 2 or 3 Stools, and afterwards used the *Decoct. Nitros.* The Blood, which had been taken away, appear'd highly inflamed. When I saw him, I found the Pustules extremely numerous un-

der the Skin, and his Body cover'd with purple Spots: Beside this, his Nose had bled profusely, and he had had several bloody Stools; or, to speak more properly, he had voided large Quantities of Blood from the *Anus*; for it was merely Blood without Excrement. His Pulse was extremely quick, but rather weak, and the Pain in his Loins violent.

The Case appearing so desperate, I determined to give him the *Bark*; a Scruple of which he took every two or three Hours, and the *Tinct. Rosar. acidulat.* for common Drink. Within 48 Hours the purple Spots disappear'd, and the Hæmorrhages stopt intirely, his Pulse grew fuller and slower, and the Pustules came on well.

From this Time to the 9th Day after the Eruption, the Appearance of the Disease continued well in every Particular; and that whole Time he persever'd in the Use of the *Bark*, &c. and for two or three Nights had taken a little *Syr. e Mecon.* On the 9th Day in the Morning I saw him well, considering the Disease: His Spirits were good, and he spit well, but very largely. About the middle of the Day he dropt asleep, when his Nurse left the Room, and carelessly stay'd away a considerable Time (as I remember, whilst she eat her own Dinner). At her Return, he was found with his Head dropt from off the Pillow, and dead, suffocated, probably, by the Quantity of Phlegm, which, his Head lying accidentally low, he could not discharge without Assistance.

Though in this first Instance my Patient unhappily miscarried, I thought I had sufficient Reason to be pleased with the Effects of this Medicine.

Soon after this I was called to a Son of Mr. *Tates*, a noted Farmer at *Hampton-Lovat* in this County. He was about twelve Years of Age, and this was the sixth Day from the first Seizure. The Pustules were confluent, and appear'd like bloody Pimples: His Breast was cover'd with *Petechiæ* and purple Spots: He had a great Itching of the Nose, so that an Hæmorrhage from thence was to be fear'd. He had been delirious for two or three Days past, and his Pulse was very weak, quick, and trembling. I order'd him *Extr. Cort. Peruv.* ʒ Scruple every third Hour, and acidulated all his Liquors with *Ol. Vitriol.* well cover'd with *Syr. Sambuc.*

The purple Spots disappear'd, after he had taken two Drachms of the *Extract* (which nevertheless was continued on thro' the whole Disease). The Pustules came on very favourably, and the Distemper afterwards, in its several Stages, more resembled the distinct Small-Pox than the confluent: Yet I never saw one where the Pustules were more numerous, and more truly confluent, not on the Face only, but the Arms and the whole Body: For, in many Parts, the Skin was raised for a considerable Extent by the included *Pus*, as if it had been done by a blistering Plaister; particularly the Arms from the Elbows to the Wrists were intirely raw; the Skin, upon letting out the Matter, coming off like a Glove in one intire Piece.

About the same time I was call'd to a young Man of 21 Years of Age in the Parish of *Elmley-Lovat* in this County, who, during the Eruption of the Small-Pox (which appeared on the second Day of his Illness), had a violent Hæmorrhage from the Nose, Purples very numerous, and excessive Pain in the

the Loins. He went well through the Disease by the same Method.—Neither of these two had any condary Fever.

In the Cases above-recited I trusted to the *Bark* alone; excepting only that I acidulated the Liquors with the vitriolic Acid; but lately I have joined Alum with it, and I think to Advantage: For, as the chief Efficacy of the *Cort. Peruv.* seem'd to me to arise from its styptic and antiseptic Qualities, whereby it not only strengthens the Solids, but gives a Firmness also to the Blood and Juices, and thereby prevents their degenerating into a putrid *Sanies*, I thought it might not unusefully be joined with Alum, a Salt of singular Virtue in the same Intentions, and which is well known to assist the *Bark* in other Cases, where it is order'd as a Styptic.

Mr. *Higgins*, of *Tedney* in *Herefordshire*, a young Man of about 24 Years of Age, after very violent Exercise in hot Weather, was seized with the previous Symptoms of the Small-Pox. The Pain in his Back particularly was very violent, and his Anxiety intolerable. On the third Day of his Illness, when I first saw him, his Skin was cover'd with purple Spots, many of them as large as a silver Penny; and his Pulse was small, and very quick. Notwithstanding this, as he was a sanguine young Man, the Weather hot, and the Sickneſs came after violent Exercise, I ventur'd to order a small Quantity of Blood to be taken from his Arm, which I found very tender and florid; and at the same time I directed for him as follows:

℞ Extr. Cort. Peruv. ℥℥. Alum. crud. ℥ii. Aq.
Cinnam. ten. ℥ vii. Syr. Cydon. ℥ i. M. cap.
coch. ii. larga alternis horis.

He had emptied this Bottle the next Day at Noon ; when I visited him again, and found him much better ; his Pulse being full and regular, and the Anxiety and Pain in his Back nearly gone.

The Small-Pox now began to shew itself, and the Purples declined apace. The Urine which he had made the Day before they had imprudently thrown away before I came ; but they informed me that it was extremely red, as if mix'd with Blood ; somewhat resembling, as they said, the Water in which fresh Meat had been washed. He had had several Stools ; so that, fearing lest that Discharge might be too violent, in the next Prescription I omitted the Alum, and substituted the *Terra Japonica* in its stead. This Mixture he continued through the whole Course of the Disease, and used no other Medicine ; excepting only that I found it necessary to give him a quieting Draught in the Evenings, containing a little *Syr. e Mecon.* a few Grains of Alum, and a Scruple of the *Extr. Cort. Peruv.*

All the bad Symptoms soon disappear'd intirely : He had no second Fever, but got thro' the Disease very easily.

Mr. *Bradford*, a Farmer at *Claines*, near this Town, about 21 Years of Age, was seized after the same manner, and got very well thro' the Disease, tho' it was confluent, by the same Method. I did not see him till the sixth Day of his Illness, and then

then his Juices were in ſo bad a State, that, beſides the *Petechiæ*, which were very numerous, a Place on his Shin-Bone, about the Size of a Crown-Piece, which had been bruifed a Day or two before his firſt Seizure, was now gangren'd; and the Edges look'd of a very deep Red, as if the Corruption was ſpreading further; his Throat likewise was ſo ſore that he could ſcarce ſwallow at all.

I order'd him the ſame Mixture as above, *viz.* *Extr. Cort. Peruv.* ℥ſſ. *Alum. crud.* ℥ii. in about half a Pint of Liquid.

He took of this two large Spoonfuls every other Hour. It was not without great Difficulty that he got down the firſt two or three Doſes; but he had Reſolution, and perſiſted; and before he had finiſhed the Bottle his Throat grew better, and he ſwallowed tolerably well. The next Day the *Petechiæ* began to fade away, and ſoon after diſappear'd intirely; the Gangrene alſo ſtopt, and in a few Days ſeparated. He continued this Medicine thro' the whole Diſeaſe, and needed no other.

Soon after Mr. *Bradford's* Recovery, his Siſter (about 19 Years of Age) who liv'd with him, was ſeized with the confluent Small-Pox, attended with innumerable *Petechiæ* and purple Spots, together with a prodigious Hæmorrhage *ab Utero*, violent Pain in the Loins, a great Loofeneſs, and Dejection of Spirits. She took the *Bark* with Alum in the ſame Method, through the whole Courſe of the Diſeaſe, and recover'd.

The *Diarrhœa* here appear'd to me to proceed from the putrid State of her Juices; and therefore, notwithstanding

notwithstanding that, I continued the Use of the Mixture, and this with good Success; for by it all the bad Symptoms were gradually mitigated, and, in a few Days, intirely carried off.

But one of the most remarkable Instances that has has come to my Knowledge, both of the Efficacy of the *Bark* in this terrible Distemper, and the whole Course of the Disease is the following.

A Servant Maid, belonging to one Mr. *Buttor*, of this Town, was seized with the previous Symptoms of the Small-Pox; but as the Pain in the Loins was excessive, the Disease was unhappily mistaken for a Fit of the Gravel; and accordingly she had been blooded plentifully, and taken Opiates and saponaceous Medicines. It was the second Day of the Eruption when I was first call'd to her. The Pustules were then extremely numerous, small, and in Appearance like a violent Itch: Her whole Body, Arms, and Legs, were cover'd with broad Spots of a deep purple Colour, many of them as large as a Sixpenny-Piece; her Eyes were red, and full of Tears, and her Countenance express'd a great Anxiety (a Symptom not easy to be described, but a terrible Prognostic in all acute Diseases); her Throat was so sore, that she swallowed with the utmost Difficulty; by Fits she was delirious, and vastly restless: She had a violent Diarrhoea, a prodigious uterine Hæmorrhage; and a very quick small Pulse; in short, she seem'd just on the Brink of the Grave.

I gave her the *Bark* with Alum, in as large a Quantity as she could get down. In about 12 Hours her Throat grew better, and she swallowed

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tolerably

tolerably well; after which she took half an Ounce of the Extract of the *Bark*, and two Scruples of Alum in 24 Hours. This Medicine I continued, without Variation, for three or four Days, when the Alum made her so sick, that I was obliged to lessen its Quantity, continuing still to give her the Extract as before.

Within this time the Purples had all disappear'd; the Hæmorrhage was stopped, and the Looseness considerably abated; the Pustules came on well, and she spit plentifully.

The Disease continued to proceed very well, till the sixteenth Day after the Eruption; but then, growing tir'd of her Medicine, she persuaded her Nurse to omit it; which she did for about 24 Hours. During this time she took but a very small Quantity of Liquids; for, as she seem'd to lie easy, and in a kind of dozing Sumbler, her Nurse thought she ought not to disturb her. It was prodigious to observe the Alteration occasion'd by this Omission and Neglect: Her Pulse grew quick and weak, and the Humours acquired the highest Degree of Putrefaction; so that she died, perfectly gangrenous, on the 20th Day.

These are a few Instances, out of many others which I could produce, of the surprising Effects of this Antidote. Indeed I have given it to very many Persons in the first Stages of the Distemper, where the *Petechiæ* have appear'd before, or as soon as the Pustules of the Small-Pox; and to others in the Time of Maturation, where the Matter has been crude and watry; and, I can safely say, almost always with

with Success. I now usually continue it thro' the whole Course of the Disease, till, the Scabbing being perfected, I find it Time to cleanse the first Passages; and sometimes I order it in the Intervals betwixt Purging for some time longer, where I find the Solids weak, or the Humours thin and acrimonious.

When I am called to a Person, and, from the Appearance of *Petechiæ*, purple Spots, Hæmorrhages, miliary Eruptions, or the like, find that the Texture of the Blood is broke, or in Danger, I immediately order the *Bark*. Nor does the Quickness of the Pulse deter me from its Use; on the contrary, I think it absolutely necessary to give it, where the Pulse is quick, if at the same time it be weak. Because, for very obvious Reasons, I judge that, in this Case, the Solids are weak, and the Fluids disposed to a putrid Acrimony.

From the Cases here recited, as well as from many others which I have met with, I think nothing more effectually or speedily cures a sore Throat in the Small-Pox than the *Bark*; nor did I ever find it at all check the Spitting in those Sorts of Small-Pox where that Evacuation is necessary.

If, in the first Stages of the Disease, the *Bark* seems to run off by Stool, so far from being prejudicial, I have commonly found it of Service. For, as † *Hoffman* well observes, that a natural Looseness often

† Alvi fluxus licet copiosus non adeo pertimescendus est ** sc. sicuti in febribus maligis petechialibus, alvi profluvium morbi facit solutionem; sic

often carries off the *Petechiæ*, so that produced by the *Cortex* herein imitates the kindly Efforts of Nature; in that respect assisting the Physician in his principal Office, which is to be *Naturæ Minister*.

In most of those, to whom I have given the *Bark*, I have found the Maturation of the Pustules forwarded by it, and the Disease shortened in its Duration; an Article surely of no mean Consideration. I commonly use the Extract (made by boiling down a Decoction of the *Bark*, without the Addition of any alkaline Salt) preferably to the gross Substance; as being, I think, of equal Efficacy, and less apt to load the Patient's Stomach. In Children and delicate Persons, who are apt to nauseate this Remedy, I have with Success given it mix'd up with thin Chocolate; which, if sufficiently sweetened, disguises it better than any thing I know of.

What the *Bark* might do, as preventive, or preparative for this Disease I know not; but in the latter Intention I should think it would prove of Use.

I suppose I need not take notice, that when I recommend the *Bark*, I would not be thought to decry or discountenance the Methods or Medicines usually order'd in this Distemper; on the contrary, I think, in so terrible a Situation, nothing ought to be

sic etiam in mali genii variolis idem evenire experientia docet. *Hoffm. de feb. variolos. Thesi. patholog. § 12.*

Alvi profluvium non nocet, quando petechiæ pariter ob valde irregularem anni constitutionem complicantur, ut potius remedio sit, et malignos humores optime expulget. *Id. ibid. Cautel. § 16.*

be omitted which can any way alleviate or assist the Patient. Dr. *Monro* has already made proper Remarks on this Head, and to his Words I refer the Reader.

I would not obtrude the foregoing Paper on the World, as containing any new Discovery ; but as I think the Efficacy of all Medicines is best to be known from repeated Trials, and communicated by accurate and authentic Histories of their Success, or these Accounts, I thought proper to offer these Observations to the Public, hoping that they may at least serve as a Commentary to the Papers already published on the Virtues of this justly celebrated Drug.

Worcester, March 10
1746-7.

I. Wall

V. *Postscript of a Letter from the Rev. Dr. Doddridge at Northampton, to Mr. Henry Baker F. R. S. of one, who had no Ear to Music naturally, singing several Tunes when in a Delirium.*

Read Nov. 12. 1747. I HARDLY know whether it be worth while to mention a little Event that happened in our Neighbourhood some time ago, which yet appear'd to me something singular in its kind.

A Clergyman's Lady, whose Husband is of some Eminence in the learned World, in a Frenzy after a Lying-in, which was quickly removed, found, during the Time of it, such an Alteration in the State and Tone of her Nerves, that, whereas she never had before or since any Ear for Music, nor any Voice, she was then capable of singing, to the Admiration of all about her, several fine Tunes, which her Sister had learned in her Presence some time before; but of which she had not then seem'd to take any particular Notice.

*Northampton, Nov. 3.
1747.*

VI. *A Catalogue of the FIFTY PLANTS from Chelsea-Gard. : presented to the Royal Society by the Company of Apothecaries for the Year 1745. pursuant to the Direction of Sir Hans Sloane Bart. Med. Reg. et Soc. Reg. nuper Præf. by Joseph Miller Apothecary. Hort. Chelf. Præf. et Prælect. Bot.*

Read Nov. 12. 1151. **A** Corus verus, five Calamus
1747. aromaticus. *Off. et C. B.*

1152. Aconitum Lycoctonum cœruleum calcari ob-
longo. *J. B.*

1153. Adonis aliàs *Eranthemum*. *ibid.*

1154. Agrimonia *Orientalis*, spica brevi crassa &c.
Tourn.

1155. Amaranthus *Siculus* spicatus. *Boccone.*

1156. Aristolochia, Clematidis recta. *Off. et C. B.*

1157. After *Atticus* cœruleus vulgaris. *ibid.*

1158. Balsamina foemina. *C. B.*

1158. Bellis radice repente, latioribus ferratis foliis.
Morison.

1160. Bidens foliis non dissectis. *Tourn.*

1161. Carthamus. *Off.* flore croceo. *ibid.*

1162. Catanance flore luteo latifolia. *ibid.*

1163. Caryophyllus *Sinensis* flore vario. *Rand. Hort.*
Chelf.

1164. Chamæcerasus *Alpina* fructu gemino rubro.
C. B.

1166. Chrysanthemum *Bermudense*, Leucoli folio
crasso. *Pluk.*

1166. Cir-

1166. *Cirsium Anglicum*. Ger.
 1167. *Cistus foemina* *Salvia* folio. C. B.
 1168. *Cistus mas Lusitanicus* folio amplissimo incano.
Tourn.
 1169. *Cistus Ladanifera Hispanica* *Salicis* folio. C. B.
 1170. *Cotyledon Africana* frutescens, flore coccineo
 umbellato. *Comelin.*
 1171. *Cytisus Alpinus* flore luteo racemoso pendulo.
 1172. *Emerus*, *Tourn.* *Colutea scorpioides*. Park.
 1173. *Ephedra major maritima*. *Tourn.*
 1174. *Elichrysum spicatum*. *ibid.*
 1175. *Fritillaria præcox purpurea variegata*. C. B.
 1176. *Fritillaria alba præcox*. *ibid.*
 1177. *Galega vulgaris* floribus cœruleis. *ibid.*
 1178. *Gentiana. Offic.* major lutea. C. B.
 1179. *Glycyrrhiza sylvestris* flore luteo pallido. *ibid.*
 1180. *Helleborus niger*, folio *Ranunculi* flore glo-
 bos. *Tourn.*
 1181. *Jasminum sive Sumbach* *Arabum*. *Alpini.*
J. B.
 1182. *Lamium rubrum* minus foliis profunde incis. *Raii Syu.*
 1183. *Lepidium latifolium*. *Off.* et C. B.
 1184. *Lotus rubra* filiqua angulosa. C. B.
 1185. *Lyfimachia lutea major* quæ *Dioscoridis*, *Off.*
 et C. B.
 1186. *Medica orbiculata*. *J. B.*
 1187. *Melilotus* *Offic.* et C. B.
 1188. *Mirabilis Peruana* flore variegato. *Park.*
Parad.
 1189. *Oenanthe Staphilini* folio aliquatenus acce-
 dens. *J. B.*

1190. *Oenanthe Apii folio.* C. B.
 1198. *Papaver hortensis semine nigro sylvest.* *Dioscorid.* *ibid.*
 1192. *Rawolfia tetraphylla latifolia.* *Plumier.*
 1193. *Refeda calcitrapæ folio.* *Morison.*
 1194. *Rosa sylvestris pomifera nostras.* *Raii Syn.*
 1195. *Salvia major; an Sphacelus* *Dioscorid?* C. B.
 1196. *Salvia minor aurita et non aurita.* *ibid.*
 1197. *Scabiosa arborea Cretica.* *Pona.*
 1198. *Statice foliis angustioribus flore rubro.* *Tourn.*
 1199. *Stoechas purpurea.* Off. et C. B.
 1200. *Thlaspi amarum arvense umbellatum.* J. B.

VIII. *A Continuation of an Account of an Essay towards a Natural History of Carolina and the Bahama Islands; by Mark Catesby F. R. S. with some Extracts out of the tenth Set, by Cromwell Mortimer Secr. R. S.*

Read Nov. 19. 1747. **T**HE Abstract of the preceding Set, which I laid before this Society in the Year 1738. is printed in the *Philosophical Transactions* N^o. 449. This tenth Set begins with Plate 100, of the second Volume. In this Part of the Work, the Author, besides Plants, has given us several Insects, particularly some remarkable Butterflies. It begins this Set with the *Shagbark-Tree*, whose Wood is of late Years become so well known here in *England*, for all sorts of Joyner's Work,

Viscum foliis longioribus, baccis rubris.

This red-berried *Mistleto* grows on the Bark of the *Mahogany* and some other Trees, as our *Mistleto* does on Apple-Trees.

82. *Bignonia Americana, capreolis donata; filiqua brevior.* Tournefort. *Inst.* This elegant Plant endures our Climate.

[In the following PLATES the Author has interspersed several remarkable Butterflies, and other Insects.]

83. *Frutex Virginianus trifolius, Ulmi samaris, Banisteri.* Pluk. *Alma.* 159. This agrees with our Climate.

Papilio caudatus maximus, Carolinianus, umbræ strisq; nigris. Pet. Mus. N°. 505.

84. *Philadelphus flore albo majore inodoro.* This agrees with the Climate of *England.*

Smilax non spinosa baccis rubris.

Phalena plumata caudata, Caroliniana virens oculata. Pet. Mus. p. 69. N°. 733.

85. *Anona fructu lutescente, levi, scratum Arietis referente.*

There are many Species of this Genus growing between the Tropics; but this only is to be found on the Northern Continent of *America.*

86. *Anona maxima, foliis oblongis angustis; fructu maximo luteo conoide; cortice glabro in areolis angulares distincto.*

Phalena magna, ex rufo et albo varia Americana.

87. *Anona foliis Lantulis, in junctura incis; fructu compresso scabro fusco, in medio acumine longo.*
The *Sappadillo* Tree.

Convolvulus foliis variis interioribus trifariam, divisis, superioribus sagittatis; floribus ex rubro purpureis.

88. *Viscum radice bulbosa; floris labello carneo, ceteris petalis sordidè luteis.*

This bulbose-rooted Plant grows only to the Trunks, and on the Limbs of Trees. Its Fibres insinuating into the Crevices of the Bark where they take such firm Rooting that great Strength is required to tear them from the Trees. They grow in the *Bahama* Islands.

Viscum Cariophylloides, Liliæ albi foliis; floris labello brevi purpureo, cæteris petalis ex luteo virescentibus.

These Plants, after the manner of the precedent, grow upon Trees on bulbose Roots, in the *Bahama* Islands.

Papillio rufa marginibus nigris punctis albis notatis.

89. *Viscum Cariophylloides angustifolium; floribus longis tubulosis cæruleis, ex spicis squamosis cæruleis erumpentibus.*

This Plant, tho' bulbose-rooted, grows to the Limbs and Branches of Trees. The Leaves are concave; the whole Plant resembling somewhat the *Manis*: What recommends this useful and very singular Plant is, that its hollow Leaves, lapping over one another, are so closely placed, that one Plant will contain two Quarts of clear Water. In many Countries between the Tropics, that are destitute of Water, having neither Springs nor Rivers, these Plants abound, and are of great Benefit in relieving

the thirsty Traveller, as (says our Author) I have often experienced in Draughts of this refreshing Water; which, tho' receiving the Heat of the Sun's perpendicular Rays, was always as cool as from a Spring. These Plants are common on many of the *Bahama* Islands, and usually grow on large Trees; particularly *Mahogany*, *Sappadillo*, *Manzanilla*, &c. which are sometimes so cover'd with them, that they seem to be the Leaves and Blossoms of the Trees on which they grow, and make a very elegant Appearance.

Locusta Caroliniana, elytris fuscis; alis interioribus nigris ad extremitates luteis.

90. *Ketmia, amplissimo Filix folio subtus argenteo, flore magno luteo.* The *Maho* Tree. Of the inner Bark of this Tree the *Musketo* Indians make their Lines both for Fishing and Striking, it being very tough and durable: It is also of great Service to the *American* Privateers, who make their Cordage and Rigging of it. (See more in *Sir Hans Sloane's Hist. Jam.* Vol. I. p. 215.)

Phalena fusca, alis superioribus Lunulis nigris notatis, inferioribus lunatis et oculatis iridibus sulphureis.

91. *Caryophyllus spurius inodorus, folio subrotundo scabro, flore racemoso hexapetaloide coccineo speciosissimo.* *Hist. Jam.* Vol. II. p. 20. T. 164.

Convolvulus minor pentaphyllos, flore purpurea.
innere.

Phalena ingens Caroliniana oculata & lateo fusca, lineis dilute purpureis insignita. The Great Moth.

92. *Phaneria flore roseo odoratissimo.* *Tourn. Inst.* This is a most elegant Plant of the *Nerium* Kind,

Kind, in great Esteem in Gardens for its Smell and Beauty.

92. *Plumeria flore nigro, foliis brevioribus obtusis.* Plum. Cat.

Granadilla, foliis Sarsaparillæ trinerviis; flore purpureo: fructu Olivæformi cæruleo. The Purple Passion-flower.

94. *Cerasus latiore folio; fructu racemoso purpureo majore.* The Pidgeon-Plum. The Fruit is ripe in December, is pleasant-tasted, and is the Food of Pidgeons, and many wild Animals.

Eruca maxima cornuta. Hist. Jam. Vol. II. p. 220. The great horned Catterpillar.

95. *Mancanilla Pyri facie.* Plumier. Plant. Americ. *Juglandi affinis arbor Julifera, &c.* Hist. Jam. Vol. II. p. 3. The Manchaneel Tree. This Wood is much esteem'd for Tables, Cabinets, and other curious Works in Joinery; but the virulent and dangerous Properties of the Sap cause a general Fear, or at least Caution, in felling them. This, says the Author, I was not sufficiently satisfy'd of, till assisting in the cutting down a Tree of this kind on *Andros* Island, I paid for my Incredulity: Some of the milky poisonous Juice spirting in my Eyes, I was two Days totally deprived of Sight, and my Eyes and Face much swell'd, and felt a violent pricking Pain the first twenty-four Hours; which from that Time abated gradually with the Swelling, and went off without any Application, or Remedy, none in that uninhabited Island being to be had. It is no Wonder that the Sap of this Tree should be so violent, when Rain or Dew falling from its Leaves on the naked Flesh causes Blisters on the Skin,

Skin, and even the *Effluvia* of it are so noxious as to affect the Senses of those which stand any time under its Shade. Other malignant Effects are commonly attributed to it, but I think with little Probability. One Charge of its pernicious Quality is, that Animals, which feed on the Fruit, are so infected by it, that Death is often the Fate of those that feed on such Animals. This is refuted in the Instance of *Guana's*, p. 64. Vol. II. The Report also of the *Baracauda's*, and other Fish, receiving their poisonous Quality by feeding on *Mançaneel* Apples, is likewise erroneous.

Viscum foliis latioribus, baccis purpureis pediculis insidentibus.

Papilio medius Gadetanus, ex nigro et sulphureo varius, maculis coccineis notatus.

96. *Prunus maritima racemosa, folio rotundo glabra; fructu minore purpureo.* Hist. Jam. Vol. II. p. 129. The *Mangrove Grape Tree*. This is a very specious Tree, producing ample stiff Leaves, on both Sides of which the *Spaniards* used to write with a Bodkin, when they were in want of Pen, Ink, and Paper. It produces a purple-colour'd pleasant Fruit resembling a Plum, the Stone of which is very astringent, and is used in Fluxes, with great Success. The Wood of this Tree makes a strong Fire, therefore used by the Privateers of *America* to harden the Steels of their Guns when faulty.

Phalena Caroliniana minor, fulva; maculis nigris alba linea, pulchre aspersa. Pet. Gaz. Nat. Tab. III. Fig. 2^a.

97. *Acacia foliis amplioribus; siliquis cinnamitis.* Plum. Cat.

Papilio

Papilio diurna prima, omnium maxima. Mouffet p. 98. Raii Hist. Insect. p. 111. Mamankanois in M S. Dni. Gualteri Raleigh penes D. Hans Sloane.

98. *Chamadaphna foliis Tini, floribus bullatis umbellatis.*

As all Plants have their peculiar Beauties, it is difficult to assign to any one an Elegance excelling all others; yet, considering the curious Structure of the Flower, and beautiful Appearance of this whole Plant, I know of no Shrub (says our Author) that has a better Claim to it; but the noxious Qualities of it lessen that Esteem which its Beauty claims: For, tho' the Deer feed on its green Leaves with Impunity, yet when Cattle and Sheep, by severe Winters deprived of better Food, feed on the Leaves of these Plants, a great Number of them die annually on the Continent of *America*. — After several unsuccessful Attempts (says the Author) to propagate it from Seeds, I procured Plants of it at several times from *America*, but with little better Success; for they gradually diminished, and produced no Blossoms; till his curious Friend Mr. Collinson, excited by a View of its dried Specimens, and Description of it, procured some Plants of it from *Pennsylvania*; which Climate being nearer to that of *England* than that from whence mine came, some ~~Plants were procured~~ in July 1740, and in 1741, in my Garden at *Wotton*.

99. *Cenchracilla arbor faxis adnascens, obrotundo, pingui folio; fructu pomiformi, in plurimas capsulas, granula siculnea stilo columnari octogono ~~prolixi adhaerenti remittente~~ diviso; Balsamum fundens.* Plum. Almag. The Balsam Tree. This Plant

Plant in *June* produces ample fair Flowers, composed of six white Petals stained with purple, surrounding the Rudiment of the Fruit, which is almost spherical, and increases to the Size of a midling Apple. From the Stalk to the Crown of the Fruit run eight Lines, like the Meridians on a Globe, from Pole to Pole. When the Fruit becomes ripe it opens at these Lines, and divides into eight Parts, disclosing its mucilaginous scarlet Seeds, which are contained in the hollow Furrows of an octagonal Core. The whole Plant is exceeding beautiful, and particularly the Structure of the Fruit in all its Parts is a most excellent Piece of natural Mechanism.

These Trees grow on Rocks, and frequently on the Limbs and Trunks of Trees, occasioned by Birds scattering or voiding the Seeds; which being glutinous like those of Mistle, take Root and grow; but not finding sufficient Nourishment to increase in Growth, the Roots spread on the Bark or Superficies of the Tree, till they find a decay'd Hole, or other Lodgment wherein is some Portion of Soil, into which they enter, and become a Tree: But the Fertility of this second Plantation being exhausted, one or more of the Roots are discharged out of the Hole, and fall directly to the Ground, tho' at forty Feet Distance, Here again they take Root, and become a much larger Tree than before. The Resin of this Tree is used for the Cure of Sores in Horses, and also instead of Tallow, for Boats and other Vessels. They grow on the *Bahama* Islands, and on many other of the hot Parts of *America*.

100. *Frutex spinosus buxi foliis, plurimis simul nascentibus; flore tetrapetaloide, pendulo, sordide flavo, tubo longissimo; fructu ovali croceo, semina parva continente.*

The Leaves of this little Tree were like those of Box; the Flowers were tubulous, of a yellow Colour about six Inches long, hanging pendulous: They were monopetalous, being very small at the *Calix*, and wide at the Mouth, in Form of a *Roman Trumpet*, except that their Verge was divided into four deep Segments, reflected back. — Shewing a Specimen of this Plant to Consul *Sherard*, who was so justly celebrated for his Knowledge in Plants, he expressed his Admiration, and declared, that had he not seen the Thing itself, he could not have believed there had been such a singular Plant in Nature.

[*Dr. Gronovius*; a very learned Gentleman at *Leyden*, and curious in botanical Studies, has paid our Author the Compliment of giving this Plant the Name of *Catesbæa*. *]

Papilio caudatus Carolinianus; fuscus, striis pallescentibus; linea et maculis sanguineis subtus ornatus. Pet. Mus. p. 50. N. 508.

* As *Langrenus* and *Riccioli* distinguished the Regions in the Moon by the Names of Men famous in Philosophy and Mathematicks, so have the Botanists paid their Compliments to Men eminent for their Knowledge of Plants, or for being Encouragers of Botany, by giving their Names to new-discover'd Plants.

VIII. *An Account of the Death of the Reverend Dr. Greene, late Rector of St. George the Martyr in Queen's Square London, and one of the Prebendaries of Worcester, where he died of an Hurt received, as he was riding out in the Neighbourhood of that City, contain'd in a Letter from Tho. Cameron M.D. to the Reverend Charles Lyttelton LL.D. and F.R.S.*

S I R,

Read Dec. 10.
1746-7.

ON Tuesday the 20th of October, about Noon, Dr. Greene's Horse, strong, nimble, and vitious, started under him, at the waving of a Plowman's Whip, and with a quick and violent Jerk, turned quite short, first to the left, and then instantly, and with the same Impetuosity, to the right. After galloping a few Paces, the Doctor fell gently off into a Hedge, without receiving any Hurt from the Fall. A Chariot was borrowed, into which he was lifted and brought home; for after this he could never stand.

At four that Evening I first saw him, just after he had been blooded. He was very faint, cold all over, and his Pulse scarce perceptible, tho' naturally very strong; the *Scrotum* so much swell'd, that the *Penis* was quite absorbed and lost in it, and its Colour a very deep Red. I ordered him a Glass of Wine with a Bit of Bread, for he had eat nothing

all that Day. This revived him, and raised his Pulse a little.

He then told me, in Answer to the Questions I put, that the Testicles were not hurt; that the twisting of the Horse gave him at that Instant the intolerable Sense of being split asunder. I answer'd, that a violent and sudden Stroke, from the Pummel of the Saddle, upon the *Os Pubis*, might probably give him that Sensation: He replied, that it did not feel like a Stroke, and still persisted in his first Expression of being split asunder.

A warm Fomentation was order'd to be constantly applied, and an Ounce and half of *Glauber's* Salt, quicken'd with two Grains of Emetic Tartar, to be given in a Quart of Gruel.

At Eleven that Night an emollient Glyster was given, the Salts having as yet done nothing. Before Morning he had six large loose Stools; but it gave him exquisite Pain to be lifted upon the Bed-pan.

Next Morning, *Wednesday*, the Swelling was increased, and the Colour deeper. I prescribed an Electuary of Bark and Salt of Amber, to prevent, if possible, the approaching Mortification.

The stale Beer Poultice was applied; and that Evening, a Fever coming on, ten Ounces of Blood were taken from his Arm.

Next Morning, *Thursday*, the Salts were repeated without the Emetic Tartar, and he had four Stools.

All this while he had made no Water, except about a Spoonful just after he was put into the Chariot. The lower Part of the Belly, where a distended Bladder would certainly point, was not swell'd, tho' the Parts upon the *Os Pubis* were very much so. The *Scrotum* increased in Bulk and bad Colour every Hour; and the Inside of the right
Thigh

Thigh grew very tumid, with great Pain, and a very perceptible Fluctuation in it.

These Observations convinced me that the Urine had found a Way into the Parts last-mention'd; tho' indeed I could not account for it, but by supposing that the *Urethra* had been bruised, even to Laceration, between the Pommel of the Saddle and the *Os Pubis*,

The Surgeon, Mr. *Russel*, soon came into my Opinion about the Urine, but imagined the Bladder must be burst. This I could not comprehend, nor could he explain; for the Bladder lies out of the Reach of all external Injury from the Causes hitherto assigned in this Case. We agreed however about Three in the Afternoon, *Thursday*, to make a Puncture into the *Scrotum*; from whence Urine, manifest to the Smell, issued pretty freely all Night.

Next Morning, *Friday*, a larger Opening was made in the right Thigh with the same Effect. The Parts subsided considerably; but the Pulse rising, *Decoctum nitrosum* was given with the Bark.

This Evening the Hiccup came on, and the *Scrotum* looked livid.

Next Day, *Saturday*, the common Emulsion, with a little Nitre, and the Extract of Bark with Musk were order'd. But the Hiccup increased, watry Blisters appear'd on the *Scrotum*, the Voice falter'd, the Head failed, and the Pulse sunk. He grew worse and worse, till he quietly expired on *Sunday* Morning at Eleven o' Clock.

Upon Dissection, we found the *Scrotum* and *Corpora cavernosa Penis* mortified; the *Ossa Pubis* wrnched asunder to the Distance of four Inches,
and

and a Rent in the Bladder, half an Inch in Length, a little above the Neck, and exactly in the middle where the *Ossa Pubis* join. This was a very astonishing Sight, and gave me quite a new Idea of the Case; which if any Physician could have discover'd without Inspection, I shall readily allow him more Penetration than I pretend to. We may now however, reason about it, with a little more Certainty than before; and it seems to me, that the Body of the Horse in twisting, acted with the Power of a Lever, to which the Suddenness of the Jerk, added in some measure the Force of Percussion. But all this leaves us still in Wonder at the Effect: For Dr. *Greene* was a very strong large-boned Man, sixty-four Years of Age, and the uniting Surface of the *Ossa Pubis* was considerably broader in him, than either the Surgeon, or I had ever seen in any Subject. I fear I have tired you; but am,

S I R,

Worcester, Dec: 2.

1747.

Your most obedient

humble Servant,

Thomas Cameron.

IX. *A Letter from the Reverend Henry Miles D. D. to Mr. Henry Baker F. R. S. concerning the Difference of the Degrees of Cold marked by a Thermometer kept within Doors, or without in the open Air.*

Dear Sir,

Read Dec. 10. 1747. I SEND you herewith an Extract from my Register of the Weather, shewing the State of my Barometer and Thermometers, for some Days of last Week: in which you will observe a sudden Change of the Temperature of the Air, particularly on *Thursday* Morning the 3d Instant, and, by the same, you may see the little Use a Thermometer is of, when kept within-doors, to determine the State of the Air abroad, as to Heat or Cold.

I have two Thermometers filled with *Mercury*, and of the same Construction, made by the late Mr. *Sisson*, in the *Strand*. The one is placed without my Chamber-Window, in a North-east Situation, under Covert, contriv'd to admit a free Passage of the Air, but to keep off Sun and Rain; the other hangs within the Window, about three Feet from the former, where the Sun never falls on it: The Room is constantly occupied, as a Bed-Chamber, but has had no Fire in it this Season.

It appears by the adjoining Table, that on *Tuesday* the 1st instant, at 8 in the Morning, the Thermometer without stood at 17 Degrees above 0. or freezing Point; that within at 14. At 9 at Night, that without was at 0. and that within at 12 above

6. So that in the Space of 13 Hours the former had fallen 17 Degrees, the latter but 2. For the other Particulars, relating to the Barometer, Wind, and Weather, I refer to the Table.

As the Barometer had been for a good while past subject to sudden considerable Variations, I suspected the severe Cold on *Wednesday* Night and *Thursday* Morning would not continue long: Accordingly, upon my observing the Thermometer without at 4 in the Morning, I found it at $\frac{9}{10}$ nine Degrees below the freezing Point, that within at $\frac{1}{2}$ five Degrees above freezing Point. But at 8 o' the Clock the same Morning, I found the Thermometer without at $3\frac{1}{2}$ three Degrees and a half above freezing, and that within at 4 Degrees above; so that in 4 Hours time, that without had risen thirteen Degrees and a half, and that within had fallen 1 Degree. This naturally led me to examine what Signs there might be of a Thaw begun, but could find none, in the Snow (which was 5 Inches deep) or in the Post, on the Windows, but within an Hour it was visible enough, and before 10 the Houses dropt. I would observe to you, that the Wind at 8 in the Morning had varied very little, if any, from what it was the Night before, *viz.* from the East, but soon after it bore to South-East and South.

May not this sudden Change of the Temper of the Air be attributed it to a subterranean Heat? And may not the shifting of the Wind be caused, in a great measure, by the same?

If you think these Observations may be acceptable to the Gentlemen of the *Royal Society*, who keep

['615]

Register of the Weather, and may serve to persuade those, who have not yet tried it, to hang their Thermometers abroad, you have Leave to communicate it from their and

Tooting, Dec. 8. *Your most obedient Servant,*
1747.

Henry Miles.

December 1747.

Days	Morning		Evening		
	Barom.	Ther.	Barom.	Ther.	
1	28 6 3	17 14 10	29 3 0	0 12 0	At 8 Morn. Wind high at S. W. much Rain preceding Night. Showery afterward in the Morning, and Wind exceeding high. Sleet at 1 $\frac{1}{2}$ p. m. calmer and clearer soon after, Wind N. W. and N. began to freeze in the Evening, clear at 9 $\frac{1}{2}$ p. m. when the Evening Account was set down.
2	29 5 5	7 0	29 4 0	2 5 0	At 8 Morn. cloudy thick Air, hard Frost, at 4 same Morn. very clear, and Glaſſes were at 29 5 0 and 2 $\frac{5}{8}$ and 4 $\frac{4}{5}$ Wind East, cloudy all Day, at 4 $\frac{1}{2}$ p. m. Snow fell, and was deep before 8. Evening Account taken at 9 $\frac{1}{2}$ p. m.
3	29 5 6	3 $\frac{1}{2}$ 0 4 $\frac{0}{10}$	29 4 9	13 $\frac{1}{2}$ 0 9 $\frac{0}{10}$	At 8 Morn. cloudy, Wind at near East, blows brisk at 4 the same Morn. Glaſſes were 29 6 1 9 and 5 $\frac{4}{5}$ Rain before 11. Evening Account at 9 $\frac{1}{2}$ p. m.

Explication.

1 Day Barom. Morn. 28 Inches $\frac{6}{10}$ - $\frac{3}{100}$.
 Ditto. Therm. Morn. $\frac{17}{10}$ is 17 Degrees above freezing Point
 the upper Number is for the Therm. without Doors, the
 lower for that in my Room, and 5 for the rest.

X. *An Account of a Child being taken out of the Abdomen, after having lain there upwards of 16 Years, during which Time the Woman had 4 Children, all born alive; by Starkey Myddleton M.D.*

Gentlemen,

Read Dec. 17. ^{*747.} **T**HE Records of your *Society* furnish us with several Cases of extra-uterine Conceptions; one of which I communicated to you *March 28. 1745.* * nevertheless I could not help flattering myself that this Case also might be worthy your Notice.

IN *April 1731.* Mrs. *Ball*, without *Bishopsgate*, perceived (by the usual Symptoms) that she was pregnant; and in *October* following, being then in the sixth Month of her Pregnancy, she had a Child died in her Lap of Convulsions, the Surprize of which occasion'd a great Fluttering within her, attended with a sensible Motion of the Child, which Motion continued, tho' gradually weaker and weaker, for about 6 or 7 Days, after which she did not perceive it to move any more; but from this time she had constant Pains attending her, which appear'd like Labour-Pains. Her Midwife, for several Days, expected a Miscarriage; but finding herself disappointed, advised her to apply to Dr. *Bamber*, whose known Abilities in the several Branches of Physick, joined to his great Experience and Judgment in

LIII 2

Midwifry,

* See *Philos. Transf.* No. 475. p. 336.

Midwifry, made him unquestionably the most proper Person to be consulted, as the Case appear'd so very uncommon in its Circumstances, at the same time that his great Humanity always gave the most free Access to the Poor in their Distresses.

The Doctor (after a proper Examination) finding sufficient Indications of a dead Child, order'd her some forcing Medicines; upon taking which about three times she discharg'd something, which the Women suppos'd to be Part of an After-birth, accompanied with a small Quantity of Water: In consequence of this Discharge her Pains ceased, but without any visible Diminution of her Belly.

After some time she again apply'd herself to the Doctor, who thought it most advisable to discontinue her Medicines, and leave the Affair intirely to Nature.

In this State she continued for about 20 Months *viz.* to *July* 1733. which was 2 Years and 3 Months from her first Reckoning, she then again apply'd herself to Dr. *Bamber*, acquainting him, that she was not yet deliver'd of the Child she so long since came to consult him about, and that her Pains were lately return'd, and daily increased without any Intermision, upon the Doctor's examining her, he thought it proper to send her home immediately, directing her to promote her Pains, by frequently sipping some warm Caudle, &c. by the Use of which her Pains became more regular, and the next Day the Doctor made her a Visit, and was inform'd she had discharg'd two Waters, but nothing more: He then carefully examined her again, and plainly felt a Child through the Integuments of the *Abdomen*, but could not give her any Assistance.

It was about this time that Dr. *Bamber* first acquainted me with the Case, and desired that I would attend her as often as Occasion might require, and that I would acquaint him, if any thing like Labour, or other remarkable Alteration should offer. Accordingly I made her a Visit, and after a proper Examination, was convinc'd of the Certainty of the Doctor's Assertion.

Her Pains now began to abate, and she grew tolerably easy; but about the latter End of *January* 1733-4. she conceived again with Child, and was deliver'd on the 28th of *October* following by Dr. *Bamber*, who sent for me to attend him in her Labour: The Doctor soon deliver'd her of a fine Boy, and after having brought away the *Placenta*, he search'd for the other Child, which he had before felt through the Integuments of the *Abdomen*, but found it was lodg'd in the Cavity of the *Abdomen*, and beyond the Reach of human Art to relieve her. This Fact every one then present was made sensible of.

October 22. 1735. I was sent for to her in her Labour, but before my Arrival she was deliver'd of a Boy; but I brought away the *Placenta*, which gave me an Opportunity of examining for the other Child, and found it in the same Situation as formerly.

October 9. 1738. I was again sent for to her, when in Labour, but she was deliver'd of a Boy before I arrived. Upon examining the Womb, and the State of the *Abdomen*, the Child appear'd just as before, without any Alteration.

June 17, 1741. I was again sent for in her Labour, but found her just deliver'd of a Girl; and
upon

upon examining the Parts, every thing appear'd as before.

October 14. 1747. being greatly emaciated by constant Pains, &c. she was admitted a Patient in *Guy's Hospital*, where she died the 7th of *November* following, after having labour'd under the Distresses and Uneasiness of carrying a dead Child within her, in a manner loose, in the Cavity of the *Abdomen* upwards of 16 Years.

The Day after her Death I open'd her, in the Presence of Dr. *Nesbit*, Dr. *Nichols*, and Dr. *Lawrence*, when the *Uterus*, and the several other Contents of the *Abdomen* appear'd (nearly) in their natural State, but on the right Side within the *Ossium* a Child presented itself, which was attach'd to the *Ilium* and neighbouring Membranes by a Portion of the *Peritoneum*, in which the *Fimbria* and Part of the right *Fallopian* Tube seem'd to lose itself.

The Child seem'd no-ways putrid; but the Integuments were become so callous, and chang'd from their natural State, that the whole seem'd to resemble a cartilaginous Mass, without Form or Distinction: The Legs indeed were distinguishable, though they were much wasted and distorted.

Upon opening the callous Integuments of the Head and Face of the Child, the Bones appear'd perfectly form'd, with a few Spots of tophous Concretions on them.

This Account may serve to convince those who are of Opinion that Boys are conceived on the right Side, and the Girls on the left; as this Woman

man had three Boys, and one Girl, after the *Fallo-
pian* Tube on the right Side had lost its Action.
I am,

Gentlemen,

With great Respect,

Decem. 17.

1747.

Your most obedient and

Most humble Servant,

Starkey Myddleton.

XI. *A Physiological Account of the Case of
Margaret Cutting, who speaks distinctly,
tho' she has lost the Apex and Body of her
Tongue: Addressed to the Royal Society,
by James Parsons M. D. F. R. S.*

Gentlemen,
Read Decem. 17. 1747. *A* ~~few~~ ^{some} of the Members of this
worthy Society were somewhat di-
vided in their Opinions concerning what was re-
ported of *Margaret Cutting*, when they were first in-
formed of her by Mr. *Baker**; it will be necessary
(in order to render her Case the better understood)
to state first, you and following, those Particulars,
which

* See these Transf. N^o. 464, Artic. II. p. 143, et seq.

which are the Result of an Examination made a few Days since by Dr. *Milward* and myself; and which, in general, differs not from the Opinion which that learned Gentleman and I mention'd to this *Society*, upon the Occasion, which the Science of Anatomy necessarily suggested to us at that time. But *James Theobald* Esq; a worthy Member of the *Royal Society*, having encouraged her to come to *London*, and having brought her to this Meeting of the *Society*, has now given us all an Opportunity of coming at the Truth of her Case; wherefore I shall now, Gentlemen, present you with, *first*, an Account of her present Condition; and then, some Considerations on the natural State and Uses of the Tongue; which will shew you how far she makes the *Lips* and *Teeth* supply the Want of *her* Tongue in speaking; and also be a Direction to every Gentleman present to judge of the Case before him.

Of her present Condition.

THE *Apex* and *Body* of the Tongue (being the only Parts that naturally fill the Cavity of the Mouth) are intirely wanting in this Woman, as closely to the Region of the *Os Hyoides*, which is the Root of the Tongue, as can well be conceived; and which is now situated too low in the Throat to be perceived, even when she opens her Mouth at the widest.

But let any one lay the Tops of the Finger and Thumb to the Sides of her Throat, and let her at the same time pronounce the Letter *k* he will feel the remaining Root of the Tongue rise towards the

the Roof of her Mouth, in order to perform; it; however, she cannot keep it there any longer than the Moment of thrusting it up, for want of the Ligament (which was destroy'd with the Tongue) that is destin'd, together with the following Muscles, to keep the whole Tongue forwards in its due Situation.

The *Genioglossi* are a Pair of Muscles which arise from the fore Part of the Inside of the lower Jaw, and are insert'd into the Body of the Tongue by three different Directions; the anterior Part is carried forward towards the *Apex*: the posterior runs obliquely backwards, towards the Root, sending a narrow Slip on each Side to the *Cornua* of the *Os Hyoides*; and the middle Part ends about the middle of the Tongue.

Now there are certain *Inequalities* appearing on, and closely adhering to the Floor of the Cavity of the Mouth, one of which being the most considerable, and having a Resemblance in its Substance to that of the Surface of the Tongue, has been, if I am rightly informed, inadvertently mistaken for a Tongue, by a Gentleman professing Surgery in the Country; and which he thought, for want of a careful Examination, perform'd the Offices proper to the *Apex*; but a little Care and Circumspection would have inform'd him, that those Appearances are only Fragments of the *Genioglossi* mention'd before, and that upon the Separation of the sound Parts from those mortified, such Fragments, as had escaped, were retract'd, and describ'd down into their proper State; nor is it difficult to conceive how the Root of the Tongue must of Necessity sink lower down into the Throat, by the Loss of these Muscles and the proper Ligament; which, as I have

M m m m

observed

observed before, naturally kept it higher than it could remain ever since their Destruction.

If the Mortification had reached the *Os Hyoides*, it must have reached, and destroy'd the Muscles of the *Larynx*, and then the Voice would have been destroy'd; and also those of the *Pharynx*, and then Deglutition could never have been perform'd; the dreadful Consequences of which need not be enumerated here; but she swallows well, and her Voice is perfect, and therefore it is not very extraordinary she should command her Voice by the proper Muscles which remain untouch'd.

The nasal Opening is quite expos'd, because the *Uvula* which cover'd it was also destroy'd; for one Pair of its Muscles (the *Glossio-Staphylini*) arise from the Tongue; by which no doubt the Distemper was communicated to this Part also.

She has her Taste perfectly; which is hereafter accounted for.

Some Considerations on the natural State and Uses of the Tongue.

THE Tongue is a fleshy Substance, chiefly made up of Muscles; and consists of, a *Basis* or Root, a *Body*, and an *Apex*; the *Basis* is the thickest, and most substantial Part, contains the *Os Hyoides*, and is naturally situated very low in the Throat: From ~~which the Body rises upwards and forwards, and is terminated by the anterior Part, an~~ *Apex*; proceeding under the *Uvula* and Roof, and lying upon the Floor (if I may so call it) of the Mouth. As to the more particular Description of all its other Parts, I ~~cannot~~ comprehend it is at all necessary here, since it is now my purpose, and would take up too much of your Time.

As to its Uses, it is said to be the Instrument of *Speaking* and *Tasting*; as to the latter, Experience shews us that the very *Apex* of the Tongue is less capable of discerning Tastes than the next Part to it, and this than the Parts yet farther back, all along the *Body* to the Root; so that altho' the Taste of any thing is first perceived by the *Apex*, yet the Gust increases, the more the Morsel approaches to Deglutition, until it is quite protruded into the *Gula*; because as the Tongue grows more thick backwards it contains more of the nervous *Papillæ* than the smaller Part, and also because there is a Capacity of tasting in the Membranes of the back Part of the Roof to the Root; as if Nature intended to increase the Gust, that Deglutition may be the better and more eagerly performed for the Service of the Animal: Hence altho' the *Apex* and *Body* of the Tongue be gone, yet there is not a Depravation of Taste, which is the Case of the Person now under your Consideration.

As to *Speech*, which is only *Sound* or *Voice* articulated into Expression, the Tongue is not the *sole* Organ for such Articulation; the *Lips*, *Teeth*, and *Roof of the Mouth* are Instruments also for the same Purpose; the two latter for the necessary Resistance to the *Apex* of the Tongue, and the Lips for the absolute Articulation and Pronunciation of many Letters, however the following short Examination of the Letters of the Alphabet, as expressed by these Organs, will demonstrate it.

The Tongue expresses some Letters with its *Apex*, and some with its Root.

Those absolutely proper to the *Apex* are only five,
d, n, l, r, t.

M m m m 2

And

And those to which it only assists are the following Letters, as *c, g, s, x, z*; all which can be performed by the Teeth alone, and which this Person does very well.

Now the Lip-Letters, and those expressed by the Root of the Tongue, she also performs as well as any Person; the former are *b, f, m, p*; and the latter are *k, q, x*; and as to the Vowels, and the Aspiration *h*, since they are chiefly founded by the Exhalation of the Voice, commanded partly by the Lips in widening or straitening the Capacity of the Mouth, these she can also express; so that there is no Letter she cannot pronounce but the five *Apex* Letters; and those she manages so well by bringing the under Lip to her upper Teeth, in the Course of her Conversation; that any one can instantly apprehend every Word she says; and she further plainly proves the Lips are a better *Sacredamentum* to the *Apex*, than that could be to the Lips if they were wanting.

Indeed it is natural enough for those who make the Tongue the absolute and sole Instrument of Speech, to imagine it is absurd to say a Woman spoke without a Tongue, as that she saw without an Eye; but when we consider the provisional assisting Organs ordain'd by the wise Author of Providence, to the utterance and expressive Articulation of Words, it will not seem so extremely marvellous, that she speaks without the *Body* and *Apex* of her Tongue, as to create any further Doubt of the Matter. I am, *Gentlemen*,

James Parson.

A N

A N
A P P E N D I X

To the FORTY-FOURTH VOLUME

OF THE

Philosophical Transactions,

Containing some Papers, which were not ready to be inserted in the Order of their Date.

- I. *Observations upon several species of small water insects of the Polypus kind, communicated in a letter to the President, from Mr. Abraham Trembley F. R. S.*

Translated from the French.

Read May 21, and June 18, 1747. I HAVE, in a Paper printed in the 47th Number of the *Philosophical Transactions*, Art. X. taken notice of several species of small water-insects, all which have been ranged in the general class of the *Polypi*. It was during

during the summer of the year 1744, that the several observations related in that paper were made, and what results from those observations, concerning the figure of these little animals, and their manner of multiplying, is sufficient, if I am not greatly mistaken, to convince any observer, that they well deserve the attention of the curious. I have accordingly since, missed no Opportunity of pursuing my enquiries concerning these insects, and whilst I have been searching for them in different waters, I have occasionally discover'd several other sorts, to which I was before a stranger; but to which I have not been able to refuse some share of my application also. The relations I found these new species of *Polypi* had to those I was already acquainted with, and several particulars in which they greatly differed, equally determin'd me to observe them all, with as much care as I was able.

This undertaking by degrees became both difficult and extensive, yet as I was well persuaded, that if it could be sufficiently pursued, it would greatly contribute, both to rectify and to enlarge our notions of Nature; I have often regretted both the want of leisure, and the want of assistance for carrying on the work. By assistance I mean the advantages I might have received from the diligence, from the abilities, and from the judgment, of such other observers, as should have been willing to joyn with me in such an undertaking.

The more minute the objects are, upon which we are to make observations, the more diffident should we be of those observations themselves. It is in these cases not sufficient to repeat such observations several times

times by one's self, but it is very proper, and frequently necessary that others should repeat them also, and should even sometimes hit upon the same themselves. And then it is, that by the means of these different observations carefully compared with each other, we may come to be better assured of the various facts we are enquiring about. Besides which it may be noted, that the number of objects, that should in this case be attended to, is really too great to be sufficiently observed by any one single person.

I believe it will not here be improper, to give the description of that *apparatus* I have made use of myself, and by which I have been enabled to make my experiments. This description will render what I have further to say the more intelligible; and will perhaps contribute to make others more capable of judging, what degree of credit should be given to the several facts, I may have occasion to relate. Those also who shall be willing to observe regularly themselves, either the small water-insects mention'd in this paper, or any others they may happen themselves to meet with, will not, I am persuaded, be displeased with the description of an *apparatus*, which they will frequently find serviceable to them in their enquiries.

The principal benefit I received from it myself was, that I was thereby enabled to observe with the several magnifiers of my microscope, small water Insects, whilst in a glass containing water sufficient to let them live therein, much in the same way as they would have done, had they still been in the ditches or other waters from whence they were first taken.

If one only proposes to one's self, to examine for some moments the figures and the motions of water-insects, one may content one's self barely with exposing such in the common way to the microscope in a few drops of water. But I dare safely assure, from divers repeated experiments, that it will often happen, with regard to several sorts of these insects, that the simple observation of them in a drop or two of water, will not be sufficient to discover all that is singular either in their shape or motion. It is therefore very proper than an observer should endeavour to examine such insects, when they are more at ease, and in a larger quantity of water. And this he will find still more necessary, if he is desirous regularly to pursue their history. For then the same insects ought to be regularly observed, for many days successively, and they ought themselves also to be as nearly as possible under the same circumstances they would have been, had they remained in the same waters, in which they naturally live.

I have accustomed myself to keep great numbers of the small insects I make observations upon, in large glasses: and it is by observing what passes in those glasses that I endeavour to discover the more general facts, relating to the natural history of these animals. After which, I have found by many repeated experiments, that it is necessary to remove into glasses of a lesser size, like that represented in *Fig. 1.* such of the insects as are to be set apart for more particular and curious microscopical observations. I put water into these glasses, from the same ditches, out of which the insects I am observing have themselves

ïelves been taken; and I shift this water more or less often as the circumstances may require.

It is easy to conceive, that to observe a small insect in one of these glasses, with a magnifier of a short *focus*, it is necessary that insect should be placed very near to one of the sides of the glass; and that it ought also to be kept steadily in the same place. The insect ought therefore to be either fixed to the side of the glass itself, or to some other body that may be conveniently so fixed. I chuse, for this purpose, substances that are slender and supple, such for example as the small branches or twigs of divers species of the *Equisetum palustre*, or water horsetail. The clustering *Polypi* are often found upon these twigs, and they may be made to settle upon them from elsewhere, as I shall take notice, by and by.

Now this is the way I take, to fix one of these twigs of horsetail against the side of my glass. Having chosen a small slip, upon which there is one or more of the clustering *Polypi* or the like, I take a piece of a peacock's feather, longer or shorter, according to the diameter of the glass I am then to use. From this piece of peacock's feather I cut away all the lateral branches or beards on both sides, excepting one at one of its extremitys; upon this one I make a knot near its insertion, but do not at first draw it close. I then bring this open knot to the small slip of the horsetail that is floating in the water of the glass, and I get one of its extremities into the knot, which I then draw close; and the slip of the horsetail is thus joyned to the piece of the feather. I next take hold of the feather, and bending it near the middle, I force its two ends (TAB. I. Fig. 4.) *b, f*, into the

N n n n

glass

glafs, I then let go the feather which I before held, and its elasticity forces its two ends againſt the ſides of the glaſs *A*, by which means the ſmall twig *dl* of the horſetail I have been ſpeaking of, and which was already fixed to the extremity of the feather *fd*, becomes alſo fixed cloſe to the ſide of the glaſs; the conſequence of which is, that the *Polypus* that is ſticking to the horſetail is obliged to remain in ſuch a ſituation, as to be within the reach of a magnifier that is but of a ſhort *focus*.

Nothing more is now wanting, but to place the magnifier before the object: for it would be both difficult and very inconvenient to hold it like a reading-glaſs in the hand. In the inſtrument *k, i, h, g, e*, I make uſe of, it is ſcrewed into a ring fixed to a ſmall branch *n, g*, which has a ball *g* at its other extremity; this ball fits a ſocket, and ſo makes a joynt, by which the fiſt branch is joyned to a ſecond *h, i*, and that again in like manner to a third *i, k*, or fourth, if there is occaſion. The foot of the whole is fitted near the edge, into a ſmall board or tablet that holds the whole *apparatus* (*Fig. 4.*). By the means of theſe joynts, the magnifier *e*, may be turned any way, and may be conveniently brought near to its proper diſtance from the object, yet as the branch which holds it, cannot well be without ſome ſpring; it will be ſtill difficult to adjust the object exactly to the *focus* of the magnifier when it is ſhort, if only the magnifier was to be moved for that purpoſe; and it will therefore be found eaſier, when the magnifier is once right againſt the object, to move gently the glaſs in which that is contained, till it is found to be preciſely in the *focus* of the magnifier:

magnifier : and for this purpose the small board upon which the glass is placed, ought to be well smoothed.

The light that comes in at a common window will be found sufficient, for observing in the water such objects as are to be seen with the bare eye, or with a hand magnifying glass; but such as must be examin'd with a *lens* of a shorter *focus*, must be view'd by the light of a taper, placed beyond the glass, and whose flame is so order'd as to be upon the level with the object.

A magnifier thus once adjusted may remain in the same place before the object, for several days together, without being disorder'd; so that, to observe the progress of the insect during all that interval; no more will be necessary, than to place from time to time a taper behind the glass, and to apply the eye to the already fixed magnifier.

Several of these *apparatus's* may be placed upon one and the same board by one another; and thus at the same time observations may be made and carried on upon different sorts of insects, or upon several insects of the same species; in order to come sooner and with more certainty at the knowledge of the facts one is enquiring about.

could never have discovered the manner in which the clustering *Polypi* are multiplied, but by the help of the expedient I have just described: and before I had the use of that *apparatus*, I only knew in general the figures of those *Polypi*, and of the clusters that contained them. I had taken notice that those clusters grew, and I had reason to suspect, that a whole cluster came from a single *Polypus*;

but I still wanted to see this increase, and to find the moment of their multiplication; for I had reason already to suspect, from what I had seen with a glass I held in my hand, that these clusters did not grow insensibly like plants; but that on the contrary, the operation I wanted to see was performed in a short portion of time. To come therefore at that moment, I resolved to observe regularly for some time *Polypi* of this sort with my Microscope, whilst they should remain in circumstances, nearly as easy and as natural to them, as those they were in in their proper habitation.

This it was that gave me the first thought of the above-described *apparatus*. And when I had prepared and fixed every thing, I set myself continually to watch for the moment of the multiplication of the clustering *Polypi*; and I then found this moment, which I had so much wished to discover, the very same morning that I began to make use of my *apparatus*.

It was, as has been seen in the paper above referred to, in that species of *Polypi*, some of which are represented in the 5th, 6th, and 7th figures of the 2d plate of the 474th number of the *Philosophical Transactions*, that I first discover'd the manner in which these small animals are multiplied: and it is indeed among several species that I am now acquainted with, one of those in which this fact is the most easy to be observed.

It is also in the same species easy to see that very odd motion, which they exhibit at their anterior extremity.

This same motion, which has also place in other species of clustering *Polypi*, is not in them so easy

to be remarked; both on account that they are lesser, and also on account that this motion itself is swifter, than in the sort above-mention'd.

There is also to be observed at the anterior extremity of several other small insects, a sort of motion which has drawn the attention of all such as have happen'd to see it, and who have almost all been curious to enquire and satisfy themselves, whether those little wheels, which appear to turn with so swift and so regular a motion, are really wheels turning upon an axis or not. This has determined me now to mention that motion, tho it is not my design to treat fully of it in this place, or to determine very precisely what I think about it: as I shall be very cautious how I assert any thing positively upon so nice a matter, until I shall have repeated again several experiments I have already made, and until I shall have tried several others.

In order to discover what this motion might really be, I have applied myself not only to observe it in the same animal placed in different altitudes, but also in different species of water-insects in which it is seen, and I have compared the *Phænomena* of all these several motions one with another. These comparisons I have found in other cases to be of singular use, and the best means of preserving myself from those illusions, which very small objects, view'd in a microscope, especially whilst they are in motion, are but too apt to present.

All I have yet learned from these comparisons, and all the other observations I have made, seem to concur in proving to me, that there is some deception of the sight in the present case, and that
the

the motion in question is not really what it at the first appears to be, a rotatory motion round an axis. And I even know some species of *Polypi*, in which this motion is, comparatively speaking, but slow: and in these it is distinctly seen, that this motion, tho' in general resembling that observed in the others, is not a revolving or rotatory motion: such, for example, is the motion which is taken notice of in that species of *Polypi*, which Mr *Leewenhoeck* has described in the 295th Number of these *Transactions*. This is one of those insects whose motion is the most to be admired, and it is besides exceedingly curious upon many other accounts.

I have already said, in my paper above referred to, that the motion in question is very slow in the clustering *Polypi*, just when they are opening again after their division, and I am greatly mistaken, if it may not then be seen very plainly that this motion is not a rotation. The same remark may also be made on the tunnel-like *Polypus*, and that, almost during all the time that it employs in its separation.

I made use of an expedient, whilst I was observing the clustering *Polypi*, whereby I was able to retard the quickness of their motion. I poured by little and little a small quantity of spirit of wine into the glass wherein they were kept. This spirit of wine immediately either abated the velocity of their motion, or took it quite away, according to the quantity of it that I poured in. That which follows both in the one and in the other of these two cases, is of use, and gives light to the present question. Sometimes the spirit of wine forces the
Polypus

Polypus entirely to draw in its lips within its body, and at other times even to detach itself entirely from its pedicle also.

Another way to take off the celerity of this motion, is to remove the insects into a water which furnishes them much more sparingly with food; fasting probably weakens them, and from their weakness arises an abatement in the quickness of their motions. This last expedient is of use and convenience for the observing of this motion whilst it is slower, for several days consecutively. And afterwards upon returning the *Polypi* into water stocked with food for them, the motion will soon be restored to its former briskness.

I remarked also the last winter, that cold deadened the motion of the clustering *Polypi*: and these animals in all probability are less voracious, and eat less in winter than they do in summer.

When the motion in the clustering *Polypi* has been retarded, either by fasting or by the cold, they become whiter or of a paler colour than before, they also then cease to multiply.

I shall not here enter into the detail of the several observations I have made, on the feeding of these clustering *Polypi*, and on the relation I have found between that and their generation; as these are particulars more properly belonging to a regular and distinct account of their natural history.

But what I now propose, is to describe, in a few words, the manner in which the clusters are formed of a certain species of *Polypi*, which multiply in the main like those represented in the figures of the 474th number of the *Philosophical Transactions*,
and

and which differ chiefly from them in the form of their clusters.

My chief end in describing here this species of *Polypi*, is to enable myself afterwards, to convey, by comparison, a distinct idea of a difference well deserving attention, that is to be observed between the manner of multiplying these, and that of another species of clustering *Polypi*, which I happened to discover the last year.

The reader will please to recollect what I have said in the foregoing paper, concerning the general manner in which clustering *Polypi* are multiplied. These little animals are nearly of a bell-like form. Their anterior extremity, in which is their mouth, and which may be looked upon as their head, is that which is hollowed inwards, and resembles the open end of the bell. Their other extremity terminates in a point, and to this point is fixed a stalk or pedicle.

The *Polypus*, when it is ready to divide, first draws in its lips into the body. It then by degrees puts on a round form, and presently after the little spherical body so formed, divides itself into two other like spherical bodies. These last in a few moments again insensibly open, they then lose their spherical form, and put on that of a bell, or of a *Polypus* as perfect and as complete, as that by the division of which it was formed. This is the manner in which several species which I have observed of clustering *Polypi* are multiplied: the whole operation is performed by that sort, of which I have spoken in my former paper, in three quarters

quarters of an hour or an hour by those I am now going to speak of.

The *Polypi* of this sort are lesser and whiter than those others, which are represented greatly magnified in the above-mentioned figures. The cluster which they form rests upon a stem easie to be remarked: this stem is fixed to some other body at its lower extremity, and from its other arise branches, making obtuse angles with the stem itself; other branches again set out from these in different places, and from these last other new ones, and so on. At the extremity of each branch may be seen a *Polypus*: and as all these branches are not of an equal length, so neither is every *Polypus*, as in the other species, at the top of the cluster, or at an equal distance from the base of the stem, but on the contrary, there are here *Polypi* to be discovered at all heights in the cluster. The assemblage of all these branches forms, together with the *Polypi* at their extremitys, a very pretty cluster or groupe, much resembling a tuft or a garland of flowers.

The stem, which carries all the cluster, and every branch in it, is capable of a remarkable sort of motion. Each will contract suddenly when it is touched, when the glass containing the cluster is moved, and even sometimes when no reason is to be perceived for their so contracting (*Fig. 6. a*). The stem and the branches contract and shorten, by disposing themselves into spirals, all whose rings nearly touch each other. Every branch is by itself capable of contraction, independently of the rest: tho it but rarely happens that any one branch does contract itself quite alone, for commonly in the action of con-

tracting it happens to touch some other branch, and then that other immediately contracts with it. When the main stem, which bears the whole cluster contracts itself, then all the branches of the cluster contract together also; and the whole becomes entirely closed. A moment after, the branches and the stem again extend themselves, and the whole cluster thereby recovers its ordinary figure. But when the cluster is considerably advanced, the stem then ceases to contract itself any more.

I shall now attempt to describe the manner in which this cluster forms itself.

A single *Polypus* detaching from the cluster, swims about in the water till it meets with some proper body to fix itself upon. It then *has* a pedicle but which is not longer than the *Polypus* itself. In the space of 24 hours this stem becomes 8 or 9 times as long as it was at the first: and it is this pedicle which is to become the main stem of the new cluster. About a day after the *Polypus* has been thus fixed, it divides itself into two. Ten or twelve hours after, these two *Polypi* again divide themselves each into two more: they soon after put out branches, and thus retire to a greater distance from each other. It is now necessary to take notice, that when two of these *Polypi* are thus formed by the division of one, the one is ordinarily much larger than the other: this larger one remains at the extremity of the branch where it was, but which branch lengthens it self more, whilst the other puts out a new branch which seems to proceed from the first. The larger of these *Polypi* again divides it self generally before the other; and all I have

have been describing is reiterated several times. Thus a principal branch is formed, provided with several lateral ones. These lateral branches become principal, with regard to those which in their turn seem to spring from them, when the *Polypi* at their extremitys come to divide. All the *Polypi* of a cluster do not detach themselves from it at the same time: those which are nearest to the origin of the branches usually detach themselves first. And every *Polypus* so detached, goes and fixes itself elsewhere, every one thus becoming at last, if not prevented, the principal of a new cluster.

I have often kept *Polypi* of this sort, in glasses of the size of that which is represented in *Fig. 4.* And the first cluster I had placed in it to observe its growth and progress, continued still well provided with *Polypi*, when there were already numbers of other clusters formed in the same glass, all which owed their being to those that had detached themselves from the first cluster. I have seen sometimes, portions of the peacock's feather in the water, entirely covered with these clusters: and I was well assured that all these clusters came from the first I had lodged in the glass. Nay I have even carried my experiments so far as to be well assured, that every *Polypus* of a cluster, as soon as detached and fixed elsewhere, became the principal of a new cluster. I mention this fact particularly, because I shall make some use of it hereafter, when I come to take notice of a difference, between this species of *Polypi* I am now treating of, and another species I shall have occasion to say somewhat about by and by.

When a cluster is already in good part stripped of its *Polypi*, the branches are no longer able to contract with the same quickness and readiness as before. When there remain but a very few *Polypi*, none but those branches to which *Polypi* are still fixed continue to exert this power; which *they* also lose as soon as they are stripped of their few remaining *Polypi*, after which they shew no further capacity of moving.

From all which particulars it seems to result, that this motion in the stem and in the branches of a cluster, is entirely derived from the *Polypi*, which are fixed upon the branches. Notwithstanding which it must be acknowledged, that an observer, attending to the appearance only of this motion, can hardly help persuading himself at the first, that they are the branches, which draw and give motion to the *Polypi*.

The resemblance and the analogy, which the figure of a cluster of *Polypi* bears to the figure of a plant, would induce any observer, for some time to imagine, that the *Polypi* which he sees fixed to the branches of the cluster, do really proceed and spring from those branches, in the same manner as the leaves, the flowers, and the fruits of a vegetable, spring from the branches of the same.

It is nevertheless the contrary of all this, that is true. The branches, composing the clusters of the *Polypi*, spring from the *Polypi* which are at their extremities. These *Polypi*, which at the first appear to be the fruits of the clusters, may more properly be considered as their roots: and of the truth of this any one may easily satisfy himself, who will
be

be at the trouble of examining regularly, and for some continuance, the whole progress of a cluster of these *Polypi*.

What further proves that these branches do really spring from the *Polypi*, and that they derive their nourishment from the same, is, that the branches constantly cease to grow, whenever the *Polypi* at their extremities are detached from them, either naturally or by any accident.

The *Polypi* of another species I am now going to speak about, form also a groupe resembling a cluster, or more properly an open flower. This flower or cluster is supported by a very distinct stem, which is by its lower extremity fixed to some of the aquatic plants or extraneous bodies that are found in the water. From the other extremity of this stem set out eight or nine branches, quite differently disposed from those of that species of *Polypi* I have been last describing. These eight or nine branches are perfectly alike, but it may be noted, that what I here call by the name of a branch, is indeed the assemblage of several other lesser branches, whose collective form much resembles that of a leaf. (*Fig. 4.*). Every one of these assemblages is composed of one principal branch or nerve, which makes with the main stem of the cluster an angle somewhat greater than a right one. From either side of this principal nerve others again set out, and these lateral ones are the less extended in length, the nearer their origin is to the extremity of their principal branch. There is a *Polypus* at the extremity of this principal branch, and another at the extremity of every one of the lateral twigs. There

There are others also on both sides of those lateral twigs, at different distances from their extremities, and these are more in number or fewer, in some proportion to the length of the twig itself. These *Polypi* are all exceedingly small, and of a bell-like figure, and they discover about their openings a quick motion, very difficult to be seen with any distinctness.

There may also be observed in several places, upon the branches of these clusters of *Polypi* (*Fig. 7.*) certain round bodies, which I at the first took for insects preying upon the *Polypi*, because I was acquainted with some such, nearly of that shape and size: but I shall presently give an account of what those round bodies really are.

Every cluster has, as I have said, eight or nine of these branches or leaves such as I have just described. They do not all of them set out from the same point; but the points from whence they do set out are not far asunder: each of these leaves is a little bent inwards, and they all form together a sort of a shallow chalice or cup. If the eye is placed right over the basis of this chalice, the appearance of the whole eight or nine branches is like unto that of a star with so many rays proceeding from the same center.

When the cluster is touched, and even frequently without it, all the branches fold together inwards, and then constitute a small round mass. The stem, which carries all the cluster, contracts also at the same time, folding it self up like a workman's measuring rule, that consists of three or four different joyns.

I saw

I saw for the first time the *Polypi* I have now been describing, on the 30th day of *May* of the last year 1746. They were upon a water-plant, which I had taken from a ditch, and disposed in one of my large glasses. They immediately struck me by their beauty, and I could not help being curious to know, in what manner such clusters were formed. The relation they bore to the species first above described, and to some other species which I had before observed, gave me reason to believe that the cluster must have sprung from a single *Polypus*, by the means of several successive divisions. I was not however contented with judging of them from analogy only; I was desirous to be actually an eye-witness of their operations; and the observations which I therefore made upon them, discover'd to me a new fact, which I should never have suspected, and which I could never have come to the knowledge of, if I had contented my self with the judgment I made of them from Analogy only.

I supposed, when I began to observe, that every cluster in question came from a single small *Polypus*, like to those with which the clusters were so plentifully provided. I therefore began by endeavouring to get one of these *Polypi* single, and fixed upon such a body as I could well dispose in my glass, so as to keep it within the reach of a magnifier of a short *focus*; and I pursued for this purpose my ordinary method.

I took some clusters of these *Polypi* well advanced, I put them apart in a glass filled with proper water to afford them sustenance; I put also into the same glass a slip of water horsetail, after I had
carefully

carefully examined it, and so assured myself that there was no *Polypus* upon it. I expected that some *Polypi* would soon detach themselves from the clusters, and that some of those *Polypi* would fix upon the horsetail, whereby I should be enabled to set them apart, and to observe in other glasses the progress of the clusters, which would, as I made no doubt, be soon produced from them.

It was on the 30th of *May*, that I set the clusters apart in the glass; on the 31st I could discover nothing new, and on the 1st of *June* I had no opportunity of observing; but on the 2d in the morning I found against the sides of the glass several small clusters of *Polypi*, of the species I am now treating of. I was surprized to find them so far advanced, for they could not have begun at the soonest before ten a clock at night, on the 30th of *May*. I saw on the 2d of *June* in the afternoon upon the slip of the horsetail, which I had placed in the same glass with the clusters of the *Polypi*, a small body, which, as I had all reason to believe was newly fixed upon it. I then took out the slip of the horsetail, and I lodged it with the small body that was upon it in another glass; after which I examined that small body with my microscope, by the help of the *apparatus* first above described.

I then found that this body was much larger than any of the *Polypi* of the present sort, and of a figure very different from them (*Fig. 8.*) This made me suppose that this body was not of the species of the *Polypi* now before us, and that it was not from any thing of this sort that I was to expect the production of a cluster of
this

this species of *Polypi*. I resolved however to continue my observations upon this minute body ; which was oblong, and had a pedicle three or four times longer than it self.

It was on the second of *June* at 5 in the evening that I put it apart in a glass, and at half an hour after 8 the same evening, I perceived that it began to split from the top towards the bottom. When the separation was accomplished, each of the two bodies, formed by this division, was nearly of the same shape as the first (*Fig. 6.*). I then thought, judging still by analogy, that it would be some time before either of these bodies would again be ready to divide ; but a very little after, I saw that they both became round, and that they disposed themselves precisely as if they were again going to separate. This novelty drew all my attention, and it again came into my mind, that this body which I had but just concluded not to contain the principle, from whence I was to expect the production of one of the clusters I was looking after, might possibly still be the very thing I was seeking for.

I now imagined that perhaps these bodys would again divide and subdivide themselves, till they should come both to the shape and to the size of the *Polypi*, which I had seen upon the clusters : I however looked upon this Idea but as a mere conjecture. The two little bodies did in effect divide presently after ; but the 4 which resulted from this division (*Fig. 7.*) had neither yet the form nor the minuteness of the *Polypi* in question. I now wanted to know whether these 4 bodies would again proceed to divide without interruption ; and I saw them a

little after again prepare for another division : this division was completed at 20 minutes after eleven, and at midnight the 8 bodies which were formed by this third division were again almost compleatly divided. The cluster was then composed of 16 *Polypi* ; and I from that moment no longer doubted, but these were clustering *Polypi* of the species I have been last describing. Among these 16 *Polypi*, there were some which had already the perfect form of those I had observed upon the more advanced clusters : and these were such as were nearest to the origin of the branches.

Few of these 16 *Polypi* were of an equal size, those which were the most distant from the origin of the branches, were the largest, and their form also was the least like to that of a bell. I found at three in the morning on the third of *June*, that the number of the *Polypi* in the cluster was considerably encreased ; they were 16 at midnight, and I could now tell 26, tho I could only see part of the cluster, the rest of it being beyond the *focus* of the microscope : and at half an hour after 7 in the morning, I counted at least 40 *Polypi*, in that same part which I could see of the cluster.

In order to judge with more certainty of the progress of the multiplication of these *Polypi* ; I counted also those of another cluster, which was so situated as to be entirely within the reach of one of my magnifiers. This cluster began to be formed about 3 in the evening of the 2d of *June* ; I mean that it was then, that the round body first began to split it self into two. At 11 the same night, that cluster consisted of 8 *Polypi*, at half an hour after 7 the
next

next morning of 64, and before night of 110 at the least. So that in about 24 hours there were formed, by repeated divisions of one single round body, no fewer than 110 *Polypi*.

The cluster I first spoke of continued to encrease from the 2d of *June* at half an hour after 8 at night, when it first began to form (it self, till the 13th, when the *Polypi* began to detach themselves from it; and there remained no more upon the cluster on the 15th.

The *Polypi* which are at the extremities of the principal branches are constantly the largest, they are those which divide themselves the most frequently, and one of the 2 *Polypi* resulting from this division is generally larger than the other. The largest remains at the end of the principal branch, whilst the lesser serves to form a lateral branch, and is it self the principal of all the *Polypi* which that lateral branch is to bear.

One can hardly now be without curiosity to know, what those round bodies really are; those sort of bulbs which contain in themselves the principle, from whence these whole clusters we are speaking of are to be produced. What gives origin to these bulbous bodies? Are they produced in the clusters by divisions and subdivisions, as the *Polypi* themselves are, which in other species are themselves the principles of the clusters? In these other species, every *Polypus* may become the principle of a cluster and of a groupe of *Polypi*, as soon as it has detached it self from the cluster where it had its origin. When one of these has once fixed alone any where and divided it self, it no ways differs either in shape

or in size, from any of the *Polypi* that were in the cluster it is now parted from, or from any of those others that will be formed in the cluster, it is by its own future division and subdivisions to produce. But how is it with the new species we are now considering? Does every *Polypus* among these, as soon as detached from the cluster, fix it self also elsewhere, and there give origin to a new cluster? Or are they only the bulbous bodies above mentioned, that have this prerogative, of being capable to produce a new colony?

These questions and doubts greatly raised my curiosity, from the time I first began to see the progress of a cluster of *Polypi*, formed by the division and the subdivisions of one of these round bulbous substances: and that which now follows, is what I have been able to collect from the various observations, and from the several experiments, which I made, whilst I was endeavouring to give my self some satisfaction with relation to the same doubts and questions.

To know, whether the *Polypi* which detach themselves from these clusters do each of them contain in themselves the principles of other new clusters, I took all the precautions I had taken in other cases, and such as I had found easily to succeed with the clustering *Polypi* of other sorts. But all was to no effect, and I could never find that any thing was produced by these *Polypi* so detached. I have therefore all reason to presume, that these *Polypi* do not contain the principles of new clusters, and it seems to me the most probable, that they all perish without ever producing any thing whatsoever.

When

When I first began to seek for the origin of the round bulbous bodies I have been speaking of, I immediately recollected those other round bodies I had before taken notice of, and which I at the first suspected to be insects preying upon these *Polypi*. I therefore again sought for them in the clusters already formed; I soon found several of them, and I perceived that they neither attacked the *Polypi* nor changed their situation. I then concluded that these round bodies were really the very bulbous ones in question, and whose origin I was now seeking for: I applied my self therefore to observe several of them, and these are the facts which I then discovered.

Some days after the clusters had begun to form themselves, I saw come out, not from the extremities of the branches, but from the bodies of the branches themselves in different places, small round buds, which grew very fast, and which arrived at their greatest size in two or three days. These bodies much resembled the galls which grow on the leaves of oaks; they were placed upon the branches of the clusters, just as those galls are usually placed upon the fibres of the leaves: and these bulbous substances do really contain the principles of the clusters.

Two or three days after these bulbs have begun to form, they detach themselves from the branches out of which they sprung, and go away swimming till they can settle upon some body, which they meet withall in the water, and to which they immediately fix themselves by a short pedicle. The bulbs are then nearly round only a little flattened on the
under

under side, the pedicles continually lengthen themselves by degrees for about 24 hours, and during the same time the bulbs also change their figure, and become nearly oval. There are in a cluster but few of these bulbs, in comparison of the great number of *Polypi* that are upon the same; neither do these bulbs all come out at the same time.

It is now easy to judge of the remarkable difference there is between the two sorts of clustering *Polypi* that are described in this paper.

The clusters of the first species of *Polypi*, and those of several others which I have also observed, do all come from *Polypi* detached from the clusters already formed. But the clusters of the *Polypi* of the second species here described, do not arise from *Polypi* detached from other clusters, but from round bodies or bulbs, larger than those *Polypi*, and of a form very different from them.

These bulbous bodies are not formed like the *Polypi*, by the division of others like themselves, but they spring from the branches of the cluster, as the flowers and the fruits of a tree spring from the branches of the same.

In diverse other species of *Polypi*, there are considerable intervals of time between their divisions. In the bulbous kind, if I may call it so, the first divisions are consecutive and follow hard upon each other, nor is there any interval of time between them, until the bodies which are to divide have already acquired the shapes of *Polypi*.

The clusters of the bulbous sort have an origin entirely different from those of the other sorts of clustering *Polypi*. Yet do these clusters enlarge, and
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the *Polypi* upon them multiply, in the same manner as those of the other species which I am acquainted with.

As I relate facts that are new, and as I am also, if I may so speak, under the necessity of mentioning new relations and analogies, I find my self under great difficulties, to find proper terms to express those relations and analogies.

I shall not here enlarge upon the analogies which may be found, between the origin of the minute animals I have been speaking of, the origin of plants, and the production of those other animals we have been hitherto more acquainted with. We shall better be able to judge of those analogies, and to compare them together, when we shall come to know more both of plants and of animals, and when we shall have made observations upon greater numbers of them.

The new and the surprizing facts, which the study of natural history lays before us more and more every day, are fully sufficient to convince us, that the nature both of plants and animals is as yet but very imperfectly known to us, and indeed much more imperfectly than many have been apt to imagine. All we do know is but very little, in comparison of what yet remains to be known: and this consideration should prompt us, still more assiduously and more diligently, to enquire after truth; as it should at the same time also make us exceedingly circumspect, and very cautious how we venture to make judgments upon the nature of things, or how we form to our selves general rules, from so few principles as we are at present masters of.

*Explanation of the figures in TAB. I. referred to,
in the foregoing Paper.*

The 4th figure in TAB. I. represents the necessary *apparatus*, for observing commodiously and regularly a *clustering Polypus* with the microscope. In the glass *A*, is the end of a peacock's feather *b, c, f*, bent at *c*, and whose extremities are by the spring of the feather, kept close against the sides of the glass. At one of the ends *f* of the feather one of its beards is left on, which is long enough to fasten to it, in *m* a slip of water horserail *dl*, upon which is a *Polypus*, which is by this means kept so close to the side of the glass, as to be within the reach of a magnifyer of a short *focus*, such as *e*. This magnifyer is screwed on to a ring whose arm *ng* has at its extremity *g* a ball playing in a socket so as to make a joynt; there are again other like joynts at *h* and *i*, and by the help of these the magnifyer may be moved every way, and be conveniently brought near to the object. The foot *ik* is stuck into the board upon which the glass is placed. The light of a window in the day-time is sufficient to observe an object so placed within the glass, either with the bare eye, or with an hand-magnifyer: but if a magnifyer of a short *focus* is necessary, the shutters must be closed, and a wax light must be placed behind the glass, at such a height as to have its light fall directly upon the object; and a magnifyer so placed may remain if there is occasion for several days in the same posture without any inconvenience.

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The 5th figure exhibits a cluster of *Polypi*, of the first of the two species described in this paper, and which is here considerably magnified.

The 6th figure shews another cluster of *Polypi* of the same sort; the number of the *Polypi* here shewn is but small, because the cluster was drawn as it appeared within 2 or 3 days after it had first begun to form it self. One of the branches of this cluster is partly contracted, and they may be seen in this situation, when a branch after contracting it self is again expanding to its ordinary state. This cluster is yet considerably more magnified than that exhibited in the 5th figure.

The 7th figure represents one branch of a cluster of *Polypi* of the second species described in this paper. There may be seen upon this branch, besides the *Polypi* which are of a bell-like form, some of those round bodies from which the clusters of this kind of *Polypi* do first spring; and which remarkably distinguish it from many other species.

The 8th figure represents one of these round or globular bodies, after it has parted it self from the cluster, has fixed it self to some other body, and after that the globule it self and its pedicle have begun to lengthen. It was in this condition on the second day of *June* at 5 in the evening.

The 9th figure exhibits the 2 bodies, that were formed by the parting of that represented in the 8th figure. This parting began at half an hour after 8, and was completed at 9 the same evening.

The 10th figure represents the four bodies, which were formed from the 2 represented in the 9th figure; and these four bodies were also formed before 10 of the clock.

II. *A collection of the magnetical Experiments communicated to the Royal Society by Gowin Knight M.B. & F.R.S. in the Years 1746 and 1747.*

I.

An account of some magnetical Experiments, exhibited before the Royal Society on Thursday the 19th of February 1746, and of which the President, who had before seen the same performed with more deliberation on the 11th of the same month, was pleased to make the following report.

Read Feb. 19. 1746-7.

BEING on *Wednesday* the 11th of this instant *February* at the house of Mr. *Knight*, I did there in company with our worthy brother *William Jones Esq;* see the following experiments; which Mr. *Knight* was desirous I should, as on this day, report to the *Society*: before whom he is also now prepared to exhibit the same, as well as the circumstances of the place and the number of the company will allow.

He first produced two almost equal bars of hardened steel, to which he had communicated a strong magnetic virtue. These bars were nearly square, each being of the length of about 15 inches and two tenths, and of the breadth and thickness of a little more than half an inch: one of these
bars

bars weighed 2 pounds and 6 pennyweight *Troy*, the other 4 pennyweight less than 2 pounds; and either of them readily lifted with one of its ends better than 3 pounds and a half.

These bars were then laid down on a table, so as to be nearly in one and the same strait line, the north pole of the one being next to the south pole of the other, and at the distance of about an inch from it: that is to say, that the north poles of both bars were pointed the same way, but without any regard to the position of the natural meridian.

Mr. *Knight* then produced a piece of natural magnet, which was one of the same he had formerly made use of, in some experiments he had before shewed to the *Royal Society*. This piece was in length an inch and $\frac{1}{10}$, in breadth $\frac{7}{10}$, and in thickness about $\frac{3}{10}$ of an inch at a medium, being considerably thicker at the one end than at the other.

This piece of magnet was then applied, so as to lie between the 2 first mentioned bars, with its thin end close to the north pole of one of them, and its thick end close to the south pole of the other. After it had lain in this position a few moments, it was taken out, and upon presenting it to the magnetic needle of a small compass box, it was observed that its thinner end, the same which had just been contiguous to the north pole of one of the bars, attracted the north end of the needle; and that the thicker end, the same which had been contiguous to south pole of the other bar, attracted the south end of the same needle.

This same piece of stone was then again put in between the bars, but in a contrary position; the thicker end now lying next to the north pole of one of

the bars, and the thinner end next to the south pole of the other. After a few moments it was again taken out, and presented as before to the compass-box: when it was found that the thin end now attracted the south end of the magnetic needle, and that the thicker end attracted the north end of the same.

The piece of stone was then again placed between the bars as at the first, and being again taken out and presented to the compass-box: the thin end was again found as at the first to draw the north end, and the thick end to draw the south end of the needle.

This same piece of magnet was then again placed between the bars, but in a position at right angles to both the former, one of its sides being now contiguous to the north pole of one of the bars, and its other side to the south pole of the other. After which being again in a few moments taken out, and presented to the compass-box as before; it was found that the side which had been in contact with the north pole of one of the bars, did attract the north end of the needle, and that the other side which had been in contact with the south pole of the other bar, did attract the south end of the same needle: whilst the two ends of the stone in which the polarity was before observed, were now found to be indifferent to either end of the needle; so that the line of direction of the poles in the stone now lay at right angles to the position in which it was situated in the former experiments.

Mr. *Knight* then produced two steel needles, of the same sort as those which are usually fixed to the cards of sea-compasses. These needles were of the length of 5 inches and $\frac{3}{16}$, and weighed severally with their caps 7 pennyweight eight, and 7 pennyweight

weight nine grains; one of these was tempered and of a blew colour, and the other was quite hard. He also produced two iron weights, severally weighing 14 pennyweight 22 grains, and 15 pennyweight 7 grains, both nearly of a cylindrical form, but with one of the ends rounded off.

The 2 large bars were then placed in a line, as in the former experiments, but with their ends so near together, as only to admit of the cap of one the needles between them.

The tempered needle was then placed flat upon the bars, so that nearly one half of it rested upon one bar, and the other half upon the other, the cap lying between the two. The needle was pressed close to the bars in this position, after which the bars were drawn away, both at the same time contrarywise, till they were clear of the needle; and this operation was repeated three or four times: after which that end of the needle which had rested upon the northern part of one of the bars, was found strongly to attract the north end of the needle in the compass-box; and the other end which had rested upon the southern part of the other bar, was found to attract in like manner the south end of the same needle in the box. The power of attraction also acquired by this needle appeared to be very considerable, it lifting easily with either of its ends, the two iron weights above mentioned, when cemented the one to the other with wax, and weighing together 1 ounce 10 pennyweights 5 grains.

The hard needle was then applied to the bars like the other and with the very same success, it lifted also, as the other had done, both the weights together.

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The two needles were then themselves applied to each other, first the northern half of the one, in a contrary direction, to the northern half of the other; and then the southern half of the first, in a like contrary direction to the southern half of the last; and from these several positions, they were severally drawn till they were clear of each other, and this several times successively: after which operation it was found, that the tempered needle had lost so far its virtue, that its northern end had hardly any effect upon the needle in the box; that its southern end even began to attract the contrary end of the needle from what it did before, and that it was no longer able to lift at either of its ends any sensible weight.

But as to the hard needle, that still retained a considerable share of its former virtue; its ends still strongly drawing the same ends of the needle in the compass-box as they drew before, and either of them lifting with ease the heavier of the two above-mentioned weights.

Mr. *Knight* then produced one of his common small magnetic bars; the which being applied to the forementioned large bars, in the same manner as the needles had been applied to the same, but in a position contrary to that of its present polarity, it had its poles thereby counterchanged or inverted, and was found to lift at that which was now become its northern end, the weight of 6 ounces 8 penny-weight and 5 grains.

He lastly produced one of his large artificial armed magnets, composed of several thin plates of steel cramped together, with which he acquainted us he had some time before lifted 36 pounds,
and

and with which he did now actually lift before us 3 1 pounds 9 ounces and three fourths.

The temper'd needle spoken of above, and which had nearly lost all its virtue, had the same again restored in great measure, upon being touched in the common way, on the armed poles of this artificial magnet; after which it discovered a strong verticity, and was able to lift at one of its ends, the heavier of the 2 abovementioned weights, that is to say somewhat more than three quarters of an ounce.

The hard needle which still retained, as has been observed, a considerable part of the virtue it had acquired by the touch of the large steel bars, was lastly touched also in a contrary sense, upon the armed poles of this artificial magnet; whereby it not only lost the polarity yet remaining, but acquired a new one the other way, it would not however after this last touch lift more than nine pennyweight.

This is the true substance of the minutes I took, when these experiments were made, and which I presume will now be verified by those Mr. *Knight* is here prepared to shew.

AFTER the reading of this report, Mr. *Knight* did accordingly produce before the *Society* the two large bars and all the other particulars therein mentioned, with which he publicly repeated all the same experiments; which notwithstanding the disadvantageous circumstances of the place, succeeded perfectly in every particular, and to the entire satisfaction of all the company.

It

It was then further proposed, that the temper'd needle, having its virtue again destroyed, should be touched upon the fine armed *Terella* belonging to the *Society*, which was the noble present of their late worthy member the Right Honourable *James* Earl of *Abercorn*, which is esteemed one of the best in *England*, and is said to have lifted in his Lordship's hands upwards of 40 pounds: the same was immediately brought, and the needle being touched therewith, was found to have acquired a strong polarity, and to lift about the same weight, as when it was before touched upon Mr. *Knight*'s large armed artificial magnet; that is to say about fifteen penny-weight.

II.

An Account of some new Experiments lately made with Artificial Magnets, by the same.

June 4, 1747.

Read July 2. 1747. **T**HE *Apparatus* for touching of Needles, which I sometime since had the Honour to shew before the *Royal Society*, was as perfect as I could have wish'd, as far as relates to the intended Use of it: But the manner in which the two Bars were disposed in their Cases made the Length of them something incommodious, especially in those of the largest Size. This made me desirous of trying if some Method could not be found out of placing the Bars parallel to each other without Danger of weakening their Force, by which means the Cases would be reduced to half their Length. I remembered that some Years ago,

ago I had tried some Experiments to this Purpose, by placing some Bars parallel and in Contact, but so that their Poles were turned different ways: in which Position I found the Virtue of some of them remain'd pretty entire, but that others were weakened thereby. I imagin'd the Reason of their losing their Force was this; that the magnetic Virtue was by degrees habituated to pass out of the Side of one Bar into that of the other in Contact with it, and thereby was hinder'd from arriving at the Ends in its full Vigour. The Reason why some suffer'd more than others was doubtless to be ascrib'd to their Difference in Temper. I repeated the Experiment about two Months ago, with a little Alteration. I placed the Bars parallel with their Poles in an alternate Position, as before, but not in Contact, having kept them at the Distance of about a Quarter of an Inch. Then I apply'd to their Ends two Pieces of soft Iron. Each Piece was laid across from the North End of one Bar to the South of the other, in the same manner as the Lifter is applied to the Feet of an armed Loadstone. The Intent of this was to draw the magnetic Virtue thereby down to the Ends of the Bars, and to convey it through the Pieces of Iron from one to the other. In this Condition I let them lie for about a Month, and then tried if they would lift the same Weight as before, which I found they did, and I thought with more Vigour. After this I repeated the Experiment with other Bars of various Sizes, and with the same Success: I have therefore now ventur'd to fit them up in Cases in the manner just described.

The Success of this Experiment had led me to another Improvement: I provided a Case of Brass that would just contain two Bars, such as are sold for half a Guinea. At one End of the Case were fixed two Feet of soft Iron, like those of an armed Loadstone, the upper Surface of which was within the Case in Contact with the Ends of the two Bars: which being parallel to each other, and their Poles in an alternate Position, the North- End of one Bar will be in Contact with one of the Feet, and the South End of the other Bar will be in like manner apply'd to the Surface of the other Foot. Upon fitting a Lifter to this new kind of Armour, I found I was able to support a Weight of about 6 Pounds: The Bars are kept asunder at the Distance of about a Quarter of an Inch, by a Slip of Wood, which slides in betwixt them.

An Instrument thus constructed seems capable of answering all the Purposes for which Loadstones are used; for when the Bars are taken out of the Case, they are fit for touching Needles, or other magnetical Uses, which may require single Bars; when in the Case, the Whole together becomes an armed Magnet, able to lift a considerable Weight. And if we want to separate iron Filings from those of other Metals, the Feet and all the lower Part of the Case will take them up in great Plenty; and by drawing the Bars a little way out of the Case the Filings will fall off.

*Some further Experiments relating to the
general Phenomena of Magnetism, by the
same.*

Read Dec. 17.
1747. **T**HE Cause of the surprizing *Phenomena* of the Loadstone has hitherto escaped our Knowledge, though diligently inquired after by Men of the Abilities. Such a Discovery is not to be made without long Experience, and a great Variety of Facts: And the Nature of the Subject is such, that the more Facts we are acquainted with, the more we find ourselves perplexed. The Conclusions we draw from some Experiments are seemingly contradicted by others: and yet these seeming Contradictions are oft-times very reconcilable upon further Experience. If what I am about to lay before the *Society* will in any-wise contribute to remove these Difficulties, I am in Hopes it will not be unacceptable, though I should not so properly explain the Nature of the Cause, as the Manner in which it acts. Many of these Experiments are not altogether new, but have not been so much attended to as they seem to deserve.

Proposition 1st.

The magnetic Matter of a Loadstone moves in a Stream from one Pole to the other internally, and is then carried back in curve Lines externally, till it arrives again at the Pole where it first entered, to be again admitted.

Experiment I.

If we lay a magnetical Body under a Piece of Paper or Glass that is strewd over with steel Filings or magnetical Sand, and by striking the Table put the Filings in Motion, they will readily dispose themselves in such a manner as to represent, with great Exactness, the Course of the magnetic Matter. Steel rendered magnetical is best for this Purpose, because it is of a more uniform Texture, than Loadstones, and will on that account exhibit a more regular Appearance. By this Experiment the curve Lines in which the magnetical Matter returns back to the Pole where it first enter'd are accurately expressed by the Arrangement of the Filings. The largest Curves are such as take their Rise from one Polar Surface, and are extended to the other; being larger in proportion as they arise nearer the Axis or Centre of the polar Surface. Those Curves which arise from the Sides of a magnetical Body are always interior to those which arise from the polar Surface; and are less and less in proportion to their Distance from the Ends. If any one should doubt, whether the magnetical Matter, which thus disposes the Filings, is really moving back in a Direction contrary to that with which it passes through the magnetical Body; let him try it in different Parts with a small Compass Needle, and the Fact will appear beyond Dispute.

Exp. II.

The larger the Distance is from Pole to Pole in different Magnets the larger will these Curves be.
This

This appears from examining Magnets of different Lengths. And this is the Reason why in the same Magnet the Curves are less in proportion to their greater Distance from the Ends of the Bars. For the Poles from whence these Curves arise are proportionably nearer each other.

Exp. III.

If the South Pole of one Magnet be opposed to the North of another, most of the magnetic Matter is carried directly out of one into the other: and does not return back in curve Lines till after having passed through both Magnets. It appears from the Arrangement of the Filings that the magnetic Matter proceeding from the polar Surface does not now diverge from the Axis as before, but runs more in straight Lines till it arrives at the polar Surface of the other Magnet. The Curves arising from the Sides, which before were bent towards the opposite End of the same Magnet, are many of them now bent the contrary Way towards the corresponding Sides of the other Magnet. Those which are not bent the contrary Way are such as are too remote from the opposed Pole of the other Magnet to be influenced thereby; and therefore continue their natural Course.

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Exp. IV.

Whilst the Bars are in the Position of the last Experiment, if a small Loadstone be placed in the Stream running from one to the other in any Position whatsoever, the Stream will pass through the Stone:

Stone: which, being again removed, will be found to have a Polarity exactly in the Direction of that Stream.

Exp. V.

If the North or South Poles of two Magnets be opposed to each other, the Filings will exhibit the Appearance of two Streams meeting; and the Curves of each will all be turned towards the opposite Pole of the same Magnet. The Appearance is altogether the same, whether the two North or two South Poles be opposed to each other. So that it is not to be determined from any of these Experiments at which of the Poles the magnetic Stream enters! As we have some Reason to think it enters at the North Pole, we may suppose that the Case, without Danger of Error; provided we build nothing upon the Supposition, but what would hold good (*mutatis mutandis*) if the contrary should be true. This being supposed, when the South Poles are opposite, the two Streams coming out at them are directly contrary, whereby the magnetic Matter is accumulated, and therefore diverges so much the faster to return back to the North Poles. When the North Poles are opposed to each other, the Streams of magnetic Matter returning from the South Poles are directly contrary, and by crowding at once towards each polar Surface are accumulated betwixt them, and converge towards them so much the faster.

The five Experiments seem sufficient to establish the Truth of the Proposition, and many more might be produced to the same Purpose.

Prop.

Prop. 2.

The immediate Cause, why two or more magnetical Bodies attract each other, is the Flux of one and the same Stream of magnetical Matter through them.

Exp. VI.

It appeared in the third Experiment, that when the South Pole of one Magnet was opposed to the North of another, a Stream of magnetic Matter was carried from one to the other, and did not return back to the Pole where it first entered, till after having passed through both Bars: and it is needless to observe that two Bars in this Position are in a State of Attraction. The fifth Experiment shewed, that when the two South or North Poles were opposed, there was no Stream common to both. Now it is well known, that magnetical Bodies in this Situation are so far from attracting, that they strongly repel each other. If the third Experiment be repeated, with the Magnets placed at different Distances from each other, we shall find, that more of the magnetical Matter will pass from one polar Surface to the other, in proportion as the Distance betwixt is less. The Attraction is therefore greater as the Distances diminish. And at Distances where none of the magnetic Stream passes from one Magnet to the other, there is no Sign of Attraction. So that this Cause is not only coexistent with the Effect, but also proportionable thereto.

Exp.

Exp. VII.

If a Piece of soft Iron which has no fixed Magnetism is any where placed in the magnetical Stream, it will be in a State of Attraction whilst it remains in that Stream, and no longer.

Exp. VIII.

A Ball of soft Iron in Contact with the Pole of a Magnet will attract a second Ball, and that a third, and so on, till the Stream becomes too weak to produce an Attraction sufficient to support a greater Weight.

Exp. IX.

Having hung a Number of Balls to each other, by applying the first to the North Pole of a Magnet, upon presenting the South of another Magnet to one of the middle Balls; all those below it will thereby be deprived of the magnetic Stream, and instantly losing their Power of Attraction fall asunder: the Ball, to which the Magnet was applied, will be attracted by it, and all the others will still remain suspended. But if the North End of a Magnet be presented, then the Ball to which it is apply'd will also drop.

Exp. X.

In a Magnet unarmed the magnetic Stream is carried back on all Sides in curve Lines to the contrary Pole,

Pole, as was seen in *Experiment I.* but when Armour is apply'd to each Pole, the magnetic Matter is thereby conducted to the Feet of the Armour; and a Lifter being thus apply'd to the Feet, the whole Stream coming out at one Pole is carried back through it to the other; by which means the Lifter is made to adhere to the Feet of the Armour with very great Force. When the Lifter is thus in Contact, the Magnet seems externally to have lost the greatest Part of its Force; though in Reality it never acted with more. If instead of the Lifter we suspend a Number of Iron Balls in Contact, they will adhere together, and hang like a Bracelet betwixt the two Feet; the returning Stream passing now through them, as before through the Lifter. Present the Pole of a Magnet, and they instantly fall asunder.

Prop. III.

The immediate Cause of magnetic Repulsion is the Conflux and Accumulation of the magnetic Matter.

It appeared in the fifth *Experiment*, that the same Poles of two different Magnets being oppos'd to each other, there was a Conflux and Accumulation of the magnetic Matter; and we find by Experience that all magnetical Bodies in a like Situation are in a State of Repulsion.

Exp. XI.

Two small Bars, the one hard, the other of a Spring Temper, being both magnetical Matter, were
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oppos'd

opposed to each other, South to South; the Filings produced the same Appearance of Repulsion, as described in the fifth *Experiment*; then the Bars being brought so near as to touch each other at the same Poles, the Repulsion was instantly changed into Attraction.

III. *A Discourse concerning the Usefulness of Thermometers in Chemical Experiments; and concerning the Principles on which the Thermometers now in Use have been constructed; together with the Description and Uses of a Metalline Thermometer, newly invented by Cromwell Mortimer M. D. Sec. R. S. &c.*

Read May 8. 1735.
here printed with some
Alterations.

CHEMISTRY being the most extensive Branch of Experimental Philosophy, hath furnish'd Mankind with the greatest Number of curious and useful Discoveries; for not only the Art of separating Metals from their Ores, of which Metals are form'd such Variety of useful Instruments, but likewise Cookery, which is so much concern'd about the Food of Mankind during Health, and also Pharmacy, which furnishes Medicines for the restoring Health when lost, the Art of Dyeing, and many other useful Manufactures, all owe their Improvements to this Science; many of which have been light on unexpectedly by the Operator, while he

he had something else in View : but in many Cases the Chemists complain, that, having once accidentally light on a curious Experiment, upon endeavouring to repeat it, they have never been able to make their Process succeed exactly, as it did the first time, notwithstanding that they made use of the same Materials, in the same Quantity, and conducted the Process thro' exactly the same Operations. Where then must the Cause of the Miscarriage lie ? Surely in the Degree of Heat made use of in the two Experiments : For, in many common Operations, how usual is it for a Preparation to be spoiled either by too little, or, most commonly, by too much Fire, too long or too short a time applied ! In order therefore to prevent these many Miscarriages, I would advise the Chemist, in his Operations, to observe his Clock with as much Exactness as the Astronomer doth in his Observations ; and in order to know to a Certainty the very Degrees of Heat he ever made use of in any Process, that so he may be able to repeat and continue the same again in any Repetitions of the same Experiment, let him have his Laboratory furnish'd with various Sorts of Thermometers, proportion'd to the Degree of Heat he intends to make use of. He will find these Instruments as useful to him in his Processes, as they have proved to the curious Gardener in his Stoves, who by them is taught to keep his Plants in the same Degrees of Heat, as are natural to them in their respective Climates ; which hath been set forth in Tables, after a very ingenious manner, by Mr.

* *Sheldrake of Norwich.* And besides the enabling him to perform his Operations with more Exactness, these Instruments would save him a great deal of Fuel; for as Liquors, while boiling, are not capable of receiving a greater Degree of Heat, all Fuel which is used more than to keep them in that State is useless; and the like happens in many other Cases.

These Instruments would also be of great Service to Maltsters, Brewers, Distillers, and Vinegar-makers; for, by Thermometers placed in different Parts of the Heap of wetted Malt, the proper Heat for its sprouting might be determined, and then regulated: The same for the Heat of the Kiln when the Malt is spread on it. By Thermometers the Brewer may ascertain the Heat of the Water when he pours it upon the Malt, the Heat of the Wort when he sets it to work, and the Heat while working: And in the like manner the Distiller and Vinegar-maker, in a Word, every Artificer, who employs Heat in his Business, may by these Instruments be certain of every Degree necessary in each Part of his Work.

Many Experiments shew, that all known Bodies, whether fluid or solid, increase their Bulk or rarefy by an Addition of Heat; and, on the contrary, contract or become more dense by the Diminution of Heat, which is the Presence of Cold: And these Alterations are always more or less sensible in proportion to the natural Rarity or Density of the Bodies.

The

* Now Truss-maker over against the End of *Suffolk-street* near *Charing-Cross*, London, 1748.

The *Air* we live in, as it is the most rare and light Fluid, so are its Alterations the most sensible; and indeed I know of no Experiments which determine how far it is capable of being expanded by Heat, or condensed by Cold; only we find that it will make its Way thro' any Fluid in which it lay dormant, when its elastic Property is roused by the Approach of such an Heat as will make the Fluid boil. On the other hand, when compress'd by a Fluid so contracted by Cold, as to freeze, or become solid, its Elasticity will only bear a certain Degree of Compression, till the Force wherewith it endeavours to restore itself, exceeds the Force by which the Parts of the Solid, that confines it, adhere to each other; and so bursts its Prison; as we often see during hard Frosts in Ice, and likewise Glass, and other hard Bodies, whose Parts cannot stretch.

Next to *Air* is *Alcohol*, or the highest rectified Spirit of Wine: This, Water, and all other Liquids, are capable of receiving no greater Degree of Heat than what makes them boil, as was first demonstrated by Monsieur *Amontons*, a Member of the *Royal Academy des Sciences* at *Paris*; but that ingenious Inventor of the Quicksilver Thermometer Mr. *Fahrenheit* hath discover'd, that when the Barometer marks a greater Pressure of the Atmosphere, the same Liquor will receive 8 or 9 Degrees more of Heat than when the Barometer is at the lowest. From hence the great Professor *Boerhaave* gives the Hint, that, from nice Experiments being made of the different Degrees of Heat mark'd by a Thermometer in boiling Water compared with the different Heights of the Barometer, and Tables formed upon

upon them, a Thermometer applied to boiling Water might, at Sea, where the Motion of the Ship hinders Observations with the Barometer, serve to determine the Difference of the Gravity of the Atmosphere. See his *Chemistry*, Tom. I. p. 171.

These, and all other Liquids, by a certain determinate Degree of Cold peculiar to each sort, lose their Fluidity, and freeze, or become solid, but not in the same Order as by Heat they boil; for by Cold Oil or Water is sooner frozen than Spirit of Wine, tho' Spirit of Wine will boil sooner than Oil or Water. All solid Bodies likewise, as Minerals, Metals, and even Stones, will become fluid, or melt, at a certain Degree of Heat peculiar to each Species; and, when thoroughly melted, it is probable they are capable of receiving no higher Degree of Heat; and, on the Absence of that Heat to a certain Degree, they all return to their natural solid State. Hence we may reasonably conclude, that Solidity is the natural State of all Bodies; and that some are only accidentally fluid, because their Constitution is such as to melt by those Degrees of Heat which our Atmosphere is most commonly subject to. All solid Bodies are observed to contract themselves into smaller Dimensions by Cold, and gradually to expand themselves at the Approach of Heat, till at last, being by Heat forc'd to the greatest Degree of Expansion, the Particles of which they are composed losing their Cohesion, they become fluid; but no Experiments have yet been made, which determine whether Solids, exposed to Cold beyond certain Degrees, will cease to contract any more.

The learned Dr. *Muschenbroek*, Prof. of Astronomy at *Utrecht*, and *F. R. S.* hath lately invented a very ingenious

ingenious Instrument, which he calls a *Pyrometer* and which Dr. *Desaguliers* hath made some Improvements to *; a full Description of which he hath given in his *Course of Experimental Philosophy*, Vol. I. p. 421. &c. By this Instrument the Elongation of Rods of several Sorts of Metals by the Approach of a certain Number of Flames of a Spirit-Lamp, and likewise their sudden Contraction, on the extinguishing one or more of those Flames, is render'd sensible to the Eye: Which sufficiently evidences the Matter of Fact, and puts it beyond all Doubt.

From the above-mention'd Property of Bodies contracting and expanding in Cold and Heat, have all Thermometers been constructed, that have ever been made use of in order to observe and compare the different Degrees of Heat, either in our Atmosphere, or in other Bodies. The most simple and most sensible of any is that aëreal Thermometer described by the great Mr. *Boyle*, in his New Experiments and Observations touching Cold, *Lond.* 1683. 4to. p. 39. It consists of a glass Bubble, with a very slender Stem not bigger than a Raven's Quill. The Bubble is left full of Air, and a few Drops of Water being convey'd into the Stem in an erect Position, will there remain suspended to a certain Height; but, by the least Addition of Heat, the Air in the Bubble expanding will push the Water up higher

* This Instrument hath since been greatly improved by that ingenious Watch-maker Mr. *John Ellicot*, F. R. S. See *Phil. Trans.* N^o, 443,

higher ; or, by the Approach of Cold, the Air contracting, the Water will fall lower in the Stem. This Instrument may be of Use in small Degrees of Heat, and in Cold, till the Water begins to freeze, when it becomes useless.

The next in Order of Sensibility is that first invented by *Cornelius Drebbelius* of *Alcmar*, and improved by *Boerhaave*. (See his *Chemistry*, Tom. I. p. 152, & 153.) It consists of an hollow glass *Lens* joined to a Stem of a larger Size than in the preceding, and a Basin into which the End of the Stem is inverted. The Air in the *Lens* must be so much rarefied, that the Stem being inverted into a tinged Liquor in the Basin, the Liquor will rise up some way in the Stem ; then, by the Application of Heat to the *Lens*, the Liquor in the Stem will be push'd down, and by Cold the Liquor will rise up. This Instrument will give Notice of the smallest Changes in the Air ; but it cannot be immersed into any Liquid for chemical Experiments, unless the Stem were made much longer, and bent down in Form of a Syphon : But even then it would be very unhandy, and, like the preceding, it would never serve for any Degree below what would freeze the Liquor made use of, nor for any above what would force out the confin'd Air through the Liquor in the Basin. Besides, both these Instruments, being subject to the Pressure of the Atmosphere, are not proper, without comparing the Barometer at the same time, to determine the Degrees of Heat at a great Distance of Time between each Experiment.

The most usual Sort of Thermometers is that described in the Account of the Experiments by the *Academy of Cimento*; which being the common ones, made of Spirit of Wine ting'd, it is needless to describe. The Bounds of the Degrees of Heat which these will measure, and which is commonly called the Range of the Instrument, are from the Degree which freezes Spirit of Wine, up to that which makes it boil. The Spirit-Thermometers, commonly made here in *London*, are so graduated, that when the Spirit is rarefied to the Degree that the most sultry Sunshine commonly known in our Climate of $51^{\circ}\frac{1}{2}$ N. Lat. can raise it, there is placed the Mark 0. or Degree of no Cold. Some few are mark'd 10 or 20 above this, if they are design'd to be used in hotter Climates; but all are graduated downwards from this: So that the 45° . is the Point of temperate, and 65° . is the Point of freezing, and 100° . is plac'd just above the Ball. But the most accurate Spirit-Thermometers are those lately made by the ingenious Mr. *Reaumur*, Member of the *Royal Academy of Sciences at Paris*; he hath taken a great deal of Pains, and used great Exactness, in fixing the certain Points of freezing of Water, of temperate Air, and boiling Water. He determines the freezing Point, by leaving his Thermometer a considerable time in Water, into which is put a good deal of Ice, at a time when the Water would not freeze of itself; and this he marks 0. or the Degree of no Heat; and his Scale is mark'd with Numbers running downwards from 0. measuring the Degrees of Cold, and upwards measuring the Degrees of Heat: At $10\frac{1}{7}$ upwards he marks

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the Point of Temperate, which he determines by placing his Instrument in a subterranean Cavern, which is neither affected by Frost nor Sunshine, but is observed to keep an equable Temperature all the Year round; such as deep Cellars and Wine Vaults commonly do. In boiling Water he finds that his Thermometer rises to his 80th Division,* or 80 Degrees, which are formed by dividing the Spirit when condensed to the freezing Point, into 1000 equal Parts; so that, with the Heat which makes Water boil, the Spirit is expanded only $\frac{80}{1000}$, more than with the Cold which freezes Water.

These Spirit-Thermometers are of Use in Experiments where somewhat greater Cold than the freezing of Water is required; but they can never be of Use in any Degrees of Heat beyond the boiling of the Spirit itself; because it then becomes volatile, or rises up in Steam, and not only expands no more, but likewise the Quantity is diminished by the Particles which fly up from the Surface of the Liquor, and are suspended in the Top of the Tube.

Many have filled their Thermometers with various Sorts of Oils†: These indeed will measure many Degrees above the boiling of Water, till they boil themselves; and then they have the same Defect as the Spirit

* But, with Submission to so great a Man, I cannot apprehend that his Thermometers, when the Spirits are raised up to 80 do mark any greater Degree of Heat than their own specific boiling Heat, which, if they are *Alcohol*, or the most rectified Spirits, answer to 174. of *Fahrenheit's* Scale; if of the Strength of common Brandy to 190.

† See Dr. *Martin's* Essays *Med. & Philos.* p. 225.

Spirit ones just mention'd, which is the Liquor losing of its Bulk by Evaporation; and they congeal much sooner than Water, and so are useless in measuring any Degrees of Cold.

The most useful Instruments, as they comprehend the largest Range, are the Mercurial Thermometers, which were brought into Use by that ingenious Artificer *Fahrenheit*, *F. R. S.* (See *Phil. Trans.* N°. 381.): But, to do Justice to a most worthy Member of the *Royal Society*, namely, Dr. *Halley*, he first gave the Hint, and even proposed the making Thermometers of Quicksilver long before *Fahrenheit's* Time (See *Phil. Trans.* N°. 197. p. 652.). However, *Fahrenheit* deserves Thanks from the World for having brought these Instruments into Use, because they will measure the greatest Degrees of Cold yet known; for no Cold hitherto observed hath been able to freeze or render *Mercury* solid: And in measuring Heat, they go far beyond boiling Water, even beyond the melting of Tin or Lead. *Fahrenheit* begins his Scale from 0. the Point to which the *Mercury* hath been observed to fall by the greatest Cold in *Island*; and computes, that the *Mercury* then * occupies 11124 Parts. This is his Point of no Heat. Then reckoning upwards from this, he finds that when the *Mercury* is rarefied only 32 Parts or Degrees more, common Water just begins to freeze: In a temperate Air it will rise to about 60. The most sultry Sunshine seldom raises it to 90; the Heat of an animal Body to 96; the boiling of *Alcohol* to 174; the boiling
of

* See *Boerh. Chem.* Tom. I. p. 174.

of Water to 212; and before the *Mercury* itself boils, it will rise to 600.

I cannot here forbear giving an Abstract of a very curious and surprising Experiment of *Fahrenheit's*, concerning the artificial Production of Cold, as it is related by *Boerhaave* in his *Chemistry*, Tom. I. p. 164. *Fahrenheit* had a *Mercurial* Thermometer made with so long a Stem, that he could carry down the Scale 76 Parts or Degrees below 0. With this Instrument he found, that Cold might be produced by gradually pouring Spirit of Nitre upon powder'd Ice, till the *Mercury* would subside to 40° below 0. that is 72° * lower than the Cold which freezes common Water. *Boerhaave*, in his *Chemistry*, Tom. I. p. 161. mentions a very pretty Way of determining the freezing Point: He advises to hang the Thermometer free in the open Air, not against any Wall or Building; and near it you must hang a Piece of very fine Linen or Muslin just dipp'd in clean Water: When this begins to grow stiff, you will find the *Mercury* stand at about the 33d Degree; and it will also stand at the same Height when an hoar Frost appears upon the Ground; which he looks upon as a certain Sign of the Beginning of freezing.

Having thus given an Account of the several Sorts of Thermometers hitherto used, and what Degrees of Heat they are proper to measure, we find none of them capable of measuring the greater Degrees of Heat, which are the most commonly made use
of

* But what is this to the marvellous natural Cold of *Siberia*, 126° . below 0? See the Preface to *Gmelin's Flora Siberica*. *Petrop.* 1747. 4to.

of by the Chemists in many of their Operations. Besides, all the above Instruments, being made of Glass, are easily broken by Accidents, and as liable to crack of themselves, by being taken out of a great Heat, and too suddenly exposed to Cold. I therefore consider'd whether the above-mention'd Property of Solids, and especially of Metals contracting with Cold, and expanding with Heat, might not be applied to the Construction of an Instrument capable of measuring all Degrees even of the greatest Cold, as well as the greatest Heat, to the melting Copper or Iron, which require more Heat than any other Metals to melt them. Altho' the Alterations in Metals are but small, in respect of those in Spirits, or even *Mercury*, yet it being found, that Iron, *e. g.* becomes $\frac{1}{60}$ longer* when red-hot, than when of its natural Temperature; and Dr. *Derham*, in his last Paper read before the *Royal Society* concerning the Vibration of *Pendulums*, says, that a Rod $39.\frac{126}{1000}$ Inches long, becomes $\frac{1}{10}$ Inch longer than its natural Dimensions in temperate Air, by being exposed to Heat equal to that of an human Body; $\frac{2}{100}$ Inch longer in hot Sunshine; that it was $\frac{2}{10}$ or $\frac{1}{5}$ Inch longer than its natural State, by being heated in a flaming Heat; that it became $\frac{7}{100}$ shorter than its natural Length by being quenched in cold Water; and still $\frac{3}{100}$ shorter, by being put into a Mixture of Salt and Snow. From which Experiments one may conclude, that from *Fahrenheit's* Cold of 40 below 0. to the greatest Heat Iron can bear without melting, a Rod of three Feet long will have about $\frac{1}{3}$ Inch Increase; which Increase of Length will be Range enough

* Vide *Sturm. Coll.*

enough to make all the intermediate Degrees observable upon an Instrument.

Suppose in *Fig. 1. TAB. II.* *AB* a Rod of Iron at its natural Length by the Heat of the Atmosphere, placed upright upon one End; upon the Point of that rests a Bar *CD* moveable on an Axis at *a*; and that, by making a Fire about the End *B* of the Rod, till it is just ready to melt, the Rod will increase in Length *Ab*, and consequently push the Bar into the Situation *cd*. Now it is obvious to any one who understands ever so little of Mechanics, that tho' the Elongation of the Rod *Ab* be even scarce perceptible to the Eye, yet if upon the Bar *CD* the Distance *aA* from the Axis to the Place where the Rod *BA* pushes against it be very small, and the other Part of the Bar *aD* very long, the Arch *Dd* may be increased at Pleasure, so as to bear to be divided into any Number of Divisions that shall be found necessary: For the Arch *Dd* will always be to the Arch *Cc* in the same Proportion as the Distance *Da* is to *aC*; and likewise the Chords of these Arches *Dd* and *Ab* will be in the same Proportion; *γ, δ*, is the Situation of the Lever on the Level; and if it be found inconvenient to make the Arm *aD* so long, as to make very minute Alterations in the Length of the Rod *AB* easily observable, this Inconveniency may be readily removed by having a second Bar *EF*, turning on the Axis *g*, whose Arm *gE* bearing up against the Extremity *D* of the first Bar or Lever, will rise with it, or be press'd down by it; and the other Arm *gF* being lengthen'd at Pleasure, the Arch *Ff* will be as large as you find convenient; or even a third and fourth Lever may be added.

When

When I first designed to have an Instrument constructed answering to the foregoing Principles of *Fig. 1.* I drew a Figure of it, wherein I proposed the Lever *AD* to have terminated in two Arches of Circles made out of one Piece of Brass; the smaller Arch formed on the Radius *aA* to be loaded with a Quantity of Metal sufficient to overcome all the Friction of the several Parts, so as to press down with a considerable Weight, and always to rest upon the Point *A* of the upright Rod *AB*; at *a* the Axis, on which they were to turn; and the larger Arch form'd on the Radius *aD*, was proposed to be a Sextant, the outward Edge of which was to be toothed, which Teeth were to play into the Teeth of a small brass Wheel carrying a steel *Index* like the Minute-hand of a Clock, which small Wheel with the Hand was to make one Revolution nearly by the utmost Rise and Fall of the Sextant at *D*; or, instead of Teeth, I proposed a Piece of a Watch-Chain to be fasten'd to the upper Limb of the Sextant, and so to be brought downwards, and passing nearly round the small Wheel in one Groove, to be fasten'd to it: In another Groove in this small Wheel was another Piece of Watch-Chain to be fasten'd, which, being passed contrarywise round the said Wheel, was to have a Weight hung to it that would be a Counterpoise to the Sextant; but, upon consulting my two ingenious Friends Mr. *Geo. Graham* and Mr. *John Ellicot* two worthy Members of this *Society*, they each of them persuaded me to lay aside that more complexed Construction, and to have the Instrument made in the plain and simple manner in which Mr.

Jackson

Jackson executed it for me in the Year 1736. as is represented in *Fig. 2* and 3. TAB. II.

The Description of the Instrument.

Fig. 2. *AB* a round Rod of Steel or Brass a Quarter of an Inch thick, and 3 Feet 1 Inch long: When the Rod is of Brass 3 Feet long, the Point *A* must be of Steel 1 Inch long, to prevent its wearing away, or losing its Point; which conical Point is made to screw on and off.

I had the first Rods made $1\frac{1}{2}$ Inch thick at *B*, and of the same Thickness 6 Inches up; but I found Inconveniencies from that Form, and that a Rod all of a Size was better.

CD, cd, are two iron Supporters, joined by a flat cross Bar at Bottom *Dd* two Inches long, in the middle of which is a Point $\frac{1}{8}$ Inch high under *B*, which goes into an Hole at the Bottom of the Rod *B*, and serves to keep the Rod in its Place at Bottom, as the cross Bar** having an Hole in it, thro' which the Rod passes, does in the middle or about $\frac{2}{3}$ up the Supporters, and the Point *A* goes into a small Hole in the under Side of the Lever; all which keep the upright Rod firm and steady in its Place. The iron Supporters are flat, or parallel to the Front of the Machine from *C* to *X* and *c* to *x*, where they are twisted half-round, so that the lower Parts *XD, xd* stand at right Angles with the upper Parts. This Contrivance gives the freer Access to the Rod for the Sand or Fluid into which the Machine is set to measure the Heat of it, the Supporters standing 2 Inches asunder at *Dd*; and that the Degrees of Heat

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may

may be compared uniformly in different Experiments, the Bottom of the Rod must always be immersed to the same Height in the Matter to be examined; and therefore I make a Mark, a small Furrow † quite round the Rod, $1\frac{1}{2}$ Inch from the Bottom *B*. For the deeper the Rod is immersed into any Matter, it will be lengthen'd the more by the same Degree of Heat.

EF, the Lever, which turns upon an Axis *G*. At *F* is fastened a String, which, passing twice round the small Pulley *H*, has a Weight *I* hanging to the other End of it, of about half a Pound, being enough to keep the String always stretch'd. At the other End *E* of the Lever is hung another Weight *L*, which must be heavy enough not only to counter-balance the longer Arm *GF*, but press down upon the Point *A* with a Weight sufficient to keep its steady.

MNO, is the back Part of the Plate, like the Dial-Plate of a Clock made of Brass. See the Front of it at *Fig. 3*.

The Pulley *H* turns upon an Axis *C* in *Fig. 3*. which goes thro' the Plate, and on the other Side, or Front of the Dial-Plate carries a Hand or Index *AB* in *Fig. 3*.

N. B. *G* being the *Fulcrum* of the Lever, the Distance *GA* being very small, and the Distance *GF* being very great, the smallest Motion at *A* will produce a very great one at *F*, and therefore the Index will turn very sensibly upon the Plate.

The Proportions of the Rod and Lever are discretionary; my Rods both of Steel and Brass are 3 Feet long in one solid Piece, but they have each a Point or Cone of Steel 1 Inch high, that screws

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upon

upon the Top at *A*. The Lever has 4 Inches from *E* to *A*, $1\frac{1}{2}$ Inch from *A* to *G*, and 12 Inches from *G* to *F*; the Distance of *G* above *c* is $1\frac{1}{2}$ Inch, the brass Pulley *H* is $\frac{2}{3}$ Inch Diameter; all the other Parts of the Machine are of Oak. The main Support or Pillar *PQ* is 1 Inch square, $2\frac{1}{2}$ Feet high, and at Bottom is let thro' a Groove at *Q* made in a great heavy Block or Pedestal of Wood *RS*. In this Groove the Pillar may be raised higher or lower, in order to adjust the Height of the Pillar to the Situation, which the Bottom of the Rod *AB* may require in different Experiments; and it is to be fixed in that Place by a Screw at *T*, which goes thro' the Front of the Block, and presses against the Bottom of the Pillar.

Fig. 3. represents the *Dial-Plate*, or Front of the Plate mark'd *MNO*: In *Fig. 2.* it is a Plate of Brass, with strong Paper glued upon it, and may be of what Size you please; mine is 11 Inches over.

AB is the Hand or *Index*, which slips on very stiff upon the Axis *C*, that carries the Pulley *H* in *Fig. 2.* The outer Circle is to be left wide enough to contain the chemical Characters or Marks which are to be made upon it, the Arch *DE* contains the Divisions of *Fahrenheit's* Mercurial Thermometer; the Arc *FG* those of *Reaumur*, or the Spirit of Wine Thermometer.

In order to adjust this Instrument for Use I place the Bottom of the Rod *B* in *Fig. 2.* immersed up to the Mark \dagger in cold River or Rain Water, in a Vessel proper to be set over the Fire; and when it

has boiled for a Quarter of an Hour, I turn the Index *AB* in *Fig. 3.* till it stands in the horizontal Position, as at *B*, being the Point of boiling-hot Water, and which answers to Division 212 on *Fahrenheit's* Arch. I then take it out of the Water, and dry it, by holding it a little over the Fire: And now great Care must be taken, that nothing alters the Situation of the Index upon the Axis; even a Nut to screw on upon the Axis at *C* may be the best to keep it fixed. If the Instrument be left to cool in the Air, the Index will fall below *B*, shewing the Degrees of Cold, or less Heat than boiling Water; and if put into melting Tin, Lead, &c. it will shew the Degrees of Heat above boiling Water. A brass Rod will serve for an Instrument to measure the greatest Degrees of Cold, and all the Degrees of Heat, to the melting of Silver or Gold; but if you have a Mind to make one to measure greater Degrees of Heat, the Rod must be of Steel, or the finest Iron. A Rod of Brass, according to Dr. *Muschenbroek's* Experiments, *l. c.* was found to lengthen 377, when one of Iron lengthen'd only 230 Parts. An iron Rod, being regulated by boiling Water, as above directed, will measure not only the Heat of melted Tin and Lead, but of Silver, Gold, and Copper, and will even shew the Degree when Iron itself begins to melt, which will be the greatest Degree of Elongation of the Rod just before the Bottom of it runs; and I imagine, that an Instrument may be constructed with Supporters, and a Rod made of Tobacco-pipe Clay, which, being regulated by boiling Mercury (for it must never touch Water), may

be adapted to measure still greater Degrees of Heat, till the Materials themselves melt into Glass.

I should advise, that not only the Scale of this sort of Thermometer, but likewise of all others, be determined by Experiments, without regarding any Equality as to Measure between the Divisions, and that in every Individual that shall be made; for a Difference in the Length and Thickness of the Rods in this sort will make a Difference in the Scale, as much or more than the Inequality in the Cavity of the Stem, or glass Tube of other Thermometers, which can never be just, if applied to a Scale whose Divisions are made equal; unless the Cavity of the Stem be perfectly equal, which it is impossible for any Workman to undertake to do, and which is very seldom, if ever, hit on by chance. Therefore, in these Instruments, let the Point *B* in *Fig. 3.* or the horizontal Position of the Index, be the Situation of the Index when the Rod has stood a Quarter of an Hour in boiling Water; there mark ∇ *boiling* on the outer Circle; on *Fahrenheit's* Arch mark 212. then set your Machine up to the Mark $+$ into melting Tin, which is the Metal that melts easiest. When the Rod is arrived to its greatest Elongation in that Metal, inscribe the Character χ on the outer Circle; do the like with Lead, and set the Character \mathfrak{z} at it. At the boiling of Mercury put the Mark \mathfrak{z} , and on *Fahrenheit's* Arch mark 600. the utmost Extent his Mercurial Instruments can measure: Then proceed to the melting of Silver, and set the Mark \mathfrak{c} ; at the melting of Gold place the Mark \odot ; at the melting of Copper place the Mark \mathfrak{z} ; at the melting of Iron place the Mark \mathfrak{d} , the most difficult to melt of all Metals.

As the Divisions pointed out by the Index will be different with Rods of different Metals or Substances, you may make different Circles upon the Plate for the Range of the different Rods, and mark them; the Iron Rod, the Brass Rod, the Clay Rod; and set the several Marks above specified upon each Circle apart; or you may, to avoid Confusion, have a different Instrument for each kind of Rod.

Being obliged to take down my *Athanor* and Wind-Furnace, upon removing twice to different Houses, and not having rebuilt them where I now live, I have not had Opportunities yet of fixing the Scale of my own Instrument, which was one Reason why I did not publish an Account of my Invention sooner; for I hope hereafter to be able to compare the Degrees of Heat necessary for the melting of each Metal, and to determine the Question whether Metals in the highest Degree of Fusion are susceptible of greater Degrees of Heat by increasing the Fire, as Water thoroughly boiling can never be made hotter; nor did I intend to have publish'd any Description of this Instrument till I had completed Tables of the Degrees of Cold and Heat, from *Fahrenheit's* Experiment of Cold produced by Art 40 Degrees below 0 to the Heat of melting Iron,

According to *Fahrenheit's* Scale, the Heat of the strongest Sunshine is at about 80. Spirit of Wine boils at 176. Water at 212. the *Lixivum* of Salt of Tartar at 240. Spirit of Nitre at 242. Oil of Vitriol at 346. Quicksilver at 600*.

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* See Augustin. Grischow *Thermometria comparata accuratius, & Harmonica*. Berolini 1740. 4to. p. 10.

As all chemical Digestions, where an equable Heat is to be continued for some time together, will come in between hot Sunshine and the boiling of Quicksilver, a Thermoscope of that Range will be sufficient for common Uses; and therefore one fitted with a brass Rod will answer these Purposes.

In large Furnaces for running down Ores, or melting great Quantities of Metal together, it is not possible to place such an Instrument; but then in Lead and Tin there may be small Outlets contrived, into which some of the melted Metal may be permitted to flow, and remain in Contact with the same Body of Metal within, where the Instrument may be placed; and for placing a Thermoscope in Iron, Copper, or Glass Furnaces, there may be a Place contrived, which shall not open into the Furnace, but have the Thickness of a Stone or Brick left between, upon which the Instrument may be plac'd; and tho' in such a Situation it will not measure the actual Heat within the Furnace, it will always give the relative or comparative Heat in the like Circumstances at different Times, and so shew us how to regulate the Heat within.

Altho' a Chemist shall have one of these Instruments to measure the Heat, he shall have used in any Experiment, and have noted down the several Digestions made use of, and the Time each lasted, he still labours under another Difficulty, which is the not being able to command any required Heat, and that it shall last a certain required Time, unless it be below that of boiling Water, which may be produced and continued by various Contrivances of Lamps, either of Spirits, or of Oil; but how to continue a Fire for 12 or 24 Hours together, without Attendance, which shall continually keep Quicksilver boiling,

boiling, Lead in Fusion, or may be let down so low as not to exceed the Heat of Sunshine, and then be raised again, and that without letting out the Fire, or moving the Vessels, may seem almost impracticable; but by an Improvement of the Furnace the antient Chemists call'd their *Athamor*, I hope to succeed in it, which may be the Subject of another Paper.

The Rev. *Stephen Hales* D. D. that most worthy Member of the *Royal Society*, to whom the World is greatly indebted for many accurate Experiments, and useful Discoveries, upon hearing the Minutes of my Paper deliver'd in to the *Royal Society*, on *May* 8. 1735. read upon the *Thursday* following, desired me to lend him the Original for some Days, telling me he had some Thoughts of making a Thermoscope with a Rod of Lead. After a few Days he returned me my Paper, with the following obliging Letter, and kind Remarks.

S I R,

IHAVE read over your *Thermometrical Tract* with Satisfaction, and believe it will be of good Use. The Want of ascertaining the Degrees of Heat and Cold is a great and important *Desideratum* in Experimental Philosophy.

What I intended to do was only this, *viz.* to get a leaden Wire, of such a Size and Strength as to bear its own Weight, to have it as long as the longest Gun-barrel I could procure, and to have it sustain a Lever as you have done; then to pour boiling Water into the Barrel, for a long time, till the Lever
rises

rises no more; the Water to have Vent at the Bottom, yet so as to have the Gun-barrel always full of Water; the Breech-Pin to be out, and the leaden Rod to rest on a Piece of Wood set upright, according to the Course of its Fibres, not sideways.

To give at the same time to a Mercurial Thermometer the Heat of boiling Water.

Then to take the freezing Point of the Leaden and Mercurial Thermometers; and afterwards to graduate all the intermediate Degrees, from the Mercurial Thermometer upon the Leaden Thermometer, as they occur.

Thus a Standard Thermometer may be made to graduate others by; but I will not now set about it, since you have undertaken the Subject.

His *Remarks* on the foregoing Paper.

Page. 673. *Thermometers* must be of excellent Use in Garden-Stoves; but foreign Plants must not be kept in an equal Degree of Heat in Stoves, to that of their native Country; *viz.* because they cannot bear as great a Heat in a confined close Air, as in an open free Air. I have been told of Coffee-Trees being killed here in *England* by this Mistake: Such Plants must doubtless be kept warm*, but not so warm as in their native Country.

P.

* I should think it best to lessen the Heat in Stoves towards the Night, and so to keep the Plants exposed to less Degrees of Heat a-nights than a-days, nay to vary the Heat daily, or to endeavour by Art to procure different Degrees of Heat, agreeable to the natural Vicissitudes of the Climate the Plants come from, having Regard both to the Seasons of the Year, and the State of flowering or Fruitification of the Plants; so that the best Way of raising Plants in Green-houses or Stoves is according to the Climates they come from; for which Mr. *Sheldrake's* Tables above-mentioned, p. 674. must be of excellent Use.

P. 676. [*All solid Bodies are observed to contract with Cold.*] I have found that Wood does not contract or dilate lengthways with Heat or Cold. I am told that Mr. *George Graham* [is about making] this Experiment, as I am also, in order to regulate *Pendulums*.

P. 682. I fear that *Boerhaave's* wet Linen, which is so thin, may begin to freeze before all the *Mercury* or Spirit of Wine in the Ball of the Thermometer has the same Degree of Cold: Tho' hanging there long before and after freezing will bring it pretty near.

P. 683. [*A Rod of Iron 3 Feet long will have about $\frac{1}{4}$ Inch Increase*] or $\frac{1}{144}$ th Part.

IV. *A Continuation of a Paper concerning Electricity, by William Watson F. R. S. printed in these Trans. N. 477, Article I. ending p. 501.*

Read Feb. 6.
1745-6.

AS Water is a Non-electric, and of consequence a Conductor of Electricity, I had Reason to believe that Ice was endowed with the same Properties. Upon making the Experiment, I found my Conjectures not without Foundation; for, upon electrifying a Piece of Ice, where-ever the Ice was touched by a Non-electric, it flashed and snapped. A Piece of Ice also held in the Hand of an electrified Man, as in the before-mentioned Processes, fired warm Spirit, chemical vegetable Oils, Camphor, and Gunpowder prepared as before. But here great Care must be taken, that,

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by the Warmth of the Hand, or of the Air in the Room, the Ice does not melt; if so, every Drop of Water therefrom considerably diminishes the received Electricity. The Experiment will succeed likewise, if, instead of the Ice, you electrify the Spirit, &c. and bring the Ice not electrified near them. I must observe, that Ice is not so ready a Conductor of Electricity as Water; so that I very frequently have been disappointed in endeavouring with it to fire inflammable Substances, when it has been readily done by a Sword, or the Finger of a Man.

In the first Paper * I had the Honour to lay before you upon this Subject, I took notice of my having observed two different Appearances of the Fire from electrified Substances; *viz.* those large bright Flashes, which may be procured from any Part of electrified Bodies, by bringing a Non-electric unexcited near them, and with which we have fired all the inflammable Substances mentioned in the Course of these Observations; and those, like the firing of wet Gunpowder, which are only perceptible at the Points or Edges of excited Non-electrics. These last also appear different in Colour and Form according to the Substances from which they proceed: For from polished Bodies, as the Point of a Sword, a Silver Probe, the Points of Scissors, and the Edges of the Steel Bar made magnetical by the ingenious Dr. *Knight*, the electrical Fire appears like a Pencil of Rays, agreeing in Colour with the Fire from *Boyle's* Phosphorus; but from un-

lished

* *Phil. Trans. Vol. 43. Page 483.*

polished Bodies, as the End of a Poker, a rusty Nail, or such-like, the Rays are much more red. The Difference of Colour here, I am of Opinion, is owing rather to the different Reflexion of the electrical Fire from the Surface of the Body from which it is emitted, than to any Difference in the Fire itself. These Pencils of Rays issue successively as long as the Bodies, from which they proceed, are exciting; but they are longer and more brilliant, if you bring any Non-electric not excited near them, tho' it must not be close enough to make them snap. If you hold your Hand at about two or three Inches Distance from these Points, you not only feel successive Blasts of Wind from them, but hear also a crackling Noise. Where there are several Points, you observe at the same time several Pencils of Rays.

It appears from Experiments, that, besides the several Properties that Electricity is possess'd of peculiar to itself, it has some in common with Magnetism and Light.

Proposition I.

In common with Magnetism, Electricity counteracts, and, in light Substances, overcomes the Force of Gravity. Like that extraordinary Power likewise, it exerts its Force *in vacuo* as powerfully as in open Air; and this Force is extended to a considerable Distance through various Substances of different Textures and Densities.

Corollary.

Gravity is the general Endcavour and Tendency of Bodies towards the Center of the Earth; this is overcome by the Magnet with regard to Iron, and

by Electricity with regard to light Substances both in its Attraction and Repulsion; but I have never been able to discern that vortical Motion, by which this Effect was said to be brought about by the late Dr. *Desaguliers* and others, having no other Conception of the Manner of its acting than as Rays from a Centre, which indeed is confirmed by several Experiments: One of which, very easy to be tried, is, that if a single downy Seed of Cotton-grass is dropped from a Man's Hand, and in its Fall comes within the Attraction of the rubbed Tube, the Down of this Seed, which before seemed to stick together, separates, and forms Rays round the Center of the Seed. Or if you fasten many of these Seeds with Mucilage of Gum Arabic round a Bit of Stick, the Down of them, when electrified, which otherwise hangs from the Stick, is raised up, and forms a circular Appearance round the Stick. As these light Bodies are directed in their Motions only by the Force impressed upon them, and as their Appearance is constantly *radiatim*, such Appearance by no means squares with our Idea of a Vortex. Some have imagined a Polarity also, when they have observed one End of an excited glass Tube repel light Substances, and the other attract them. But this is a Deception arising from the whole Length of the Tube not being excited, but only such Part of it as has been rubbed; so that as much of the Tube as is held in the Hand remains in an unexcited State, and permits light Substances to lie still thereon, though forcibly repell'd at the other End. This attractive Power of Electricity acts not only upon Non-electrics, as Leaf-Gold, Silver, Thread, and such-like, but

but also upon Originally-Electrics, as Silk, dry Feathers, little Pieces of Glass, and Resin : it attracts all Bodies, that are not of the same Standard of Electricity (if I may be allowed the Expression) as the excited Body from which it proceeds. I have found no Body, however dense, whose Pores are not pervious to Electricity by a proper Management, not even Gold itself.

Proposition II.

In common with Light, Electricity pervades Glass, but suffers no Refraction therefrom; I having, from the most exact Observations, found its Direction to be in right Lines, and that through Glasses of different Forms, included one within the other, and large Spaces left between each Glass.

Corollary.

This rectilinear Direction is observable only as far as the Electricity can penetrate through unexcited Originally-Electrics, and those perfectly dry; nor is it at all material, whether these Substances are transparent, as Glass; semidiaphanous, as Porcelain, or thin Cakes of Wax; or quite opaque, as thick woollen Cloth, as well as woven Silk of various Colours; it is only necessary that they be Originally-Electrics. But the Case is widely different with regard to Non-electrics; wherein the Direction, given to the Electricity by the excited Originally-Electric, is alter'd as soon as it touches the Surface of a Non-electric, and is propagated with a Degree of Swiftness scarcely to be measured in all possible Directions to impregnate the whole non-electric Mass in Contact with it, or nearly so, however different in itself, and which must of Necessity be terminated by an Originally-

Originally-Electric, before the Electricity exerts the least Attraction ; and then this Power is observed first at that Part of the Non-electric the most remote from the Originally Electric. Thus, for Example, by an excited Tube held over it, Leaf-Gold will be attracted thro' Glass, Cloth, &c. held horizontally in the Hand of a Man standing upon the Floor, and this Attraction is exerted to a considerable Distance. On the contrary, the rubbed Tube will not attract Leaf-Gold, or other light Bodies, however near, through Silver, Tin, the thinnest Board, Paper, or any other Non-Electric, held in the manner before-mention'd. But if you rub the Paper over with Wax melted, and by that means introduce the Originally-Electric therein, you observe the Electricity acts in right Lines, and attracts powerfully. And here I must beg Leave to remind you, not only of the former Corollary, but of some of the former Experiments also ; by which it appears, that although, to make a Non-Electric exert any Power, we must excite the whole Mass thereof, yet we can excite what Part, and what only, of an Originally-Electric we please. Thus we observe, that Leaf-Gold, and the Seed of Cotton-Grass (which grows upon Bogs, and is a very proper Subject for these Inquiries) are attracted under a glass Jar made warm*, and turned Bottom upwards, upon which are placed Books, and several other

* I have constantly observed, that the electrical Attraction through Glass is much more powerful when the Glass is made warm, than when cold. This Effect may proceed from a twofold Cause : First, warm Glass does not condense the Water from the Air, which makes the Glass, as has been before demonstrated (p. 111) a Conductor of Electricity : Secondly ; As Heat enlarges the Dimensions of all known Bodies, and consequently causes their constituent Parts to recede from each other, the electrical Effluvia, passing in straight Lines, find probably a more ready Passage thro' their Pores.

other Non-Electrics; and that the Motions of the light Bodies underneath correspond with the Motions of the glass Tube held over them, the Electricity seeming instantaneously to pass thro' the Books and the Glass. But this does not happen, till the Electricity has fully impregnated the Non-electrics, which lie upon the Glass; which received Electricity is stopped by the Glass; and then these Non-Electrics dart their Power directly through the upper Part of the Glass, after the Manner of Originally-Electrics. But if the thinnest Non-Electric, even the finest Paper, as I before mentioned, is held in the Hand of a Man at the smallest Distance over the the Leaf-Gold, and the Electricity is not stopped, not the least Power will be exerted, and the Gold will lie still. I must here remark likewise, that this Law of Electricity is so constant and regular, that I have not found one Deviation from it; so that even the Quicksilver, spread thin, as it usually is at the Back of a Plate of a Looking-Glass, will prevent the passing through of the electrical Attraction, unless stopped by an Originally-Electric. This Penetration of the electrical Power through Originally-Electrics is much greater than has hitherto been imagined, and has caused the Want of Success to great Numbers of Experiments. I have been at no small Pains to determine, how far this Power can penetrate through a dry Originally-Electric, and have found, by repeated Trials, that either in a Cake of Wax alone, or of Wax and Resin mixed, when the Electricity is very powerful, it has passed, I say, in strait Lines through these Cakes of the Thickness of two Inches and $\frac{4}{10}$; but I never could make it act through one of two Inches $\frac{8}{10}$; for in this it was perfectly

stopped. So that the Cakes commonly made use of to stop the Electricity, by being too thin, suffer a considerable Quantity of the electrical Power to pervade them, and be lost in the Floor. I make no Doubt, if the electrical Power could be more increased, it would penetrate much further through these Originally-electric Bodies.

Proposition III.

Electricity, in common with Light likewise, when its Forces are collected, and a proper Direction given thereto upon a proper Object, produces Fire and Flame.

Corollary.

The Fire of Electricity (as I have before observ'd) is extremely delicate, and sets on Fire, as far as I have yet experienced, only inflammable Vapours. Nor is this Flame at all heighten'd by being superinduced upon an iron Rod, red-hot with coarser culinary Fire, as in a preceding Experiment; nor diminish'd by being directed upon cold Water. However, I was desirous of knowing if this Flame would be affected by a still greater Degree of Cold; and in order to determine this, I made an artificial Cold, by which the Mercury, in a very nice Thermometer adjusted to *Fahrenheit's* Scale, was depressed in about 4 Minutes from 15 Degrees above the freezing Point to 30 Degrees below it, that is, the Mercury fell 45 Degrees. From this cold Mixture, when electrified, the Flashes were as powerful, and the Stroke as smart, as from the red-hot Iron. I could have made the Cold more intense, but the above was sufficient for my Purpose. This Experiment

ment seems to indicate, that the Fire of Electricity is affected neither by the Presence or Absence of other Fire. For, as red-hot Iron, by Sir *Isaac Newton's* Scale of Heat, is fixed to 192 Degrees, and as the Ratio between Sir *Isaac's* Degrees and *Fahrenheit's* is as 34 to 180, it necessarily follows, that the Difference of Heat between the hot Iron and the cold Mixture is 1040 Degrees; and nevertheless, this vast Difference makes no Alteration in the Appearance of the electrical Flame. We find likewise, that as the Fire, arising from the Refraction of the Rays of Light by a *Lens*, and brought to a *Focus*, is observed first at some small Distance from their Surfaces, to set on fire combustible Substances; the same Effect, as I have before observ'd, is produced in like manner by electrical Flame.

I may perhaps be thought too minute in some of the before mention'd Particulars; but in Inquiries abstruse as these are, where we have so little *a priori* to direct us, the greatest Attention must be had to every Circumstance, if we are truly desirous of investigating the Laws of this surprising Power. For, as has been said upon another Occasion, by my ever honour'd Friend *Martin Folkes* Esq; our most worthy President, " That Electricity seems to furnish an inexhaustible Fund for Inquiry; and sure *Phenomena* so various and so wonderful can arise only from Causes very general and extensive, and such as must have been designed by the Almighty AUTHOR of NATURE for the Production of very great Effects, and such as are of great Moment to the System of the Universe."

If these Observations receive the Countenance of this Learned *Society*, I shall think myself sufficiently recompensed; and am,

Gentlemen, with the highest Esteem,

Your most obedient humble Servant,

W. Watfon.

V. *A Sequel to the Experiments and Observations tending to illustrate the Nature and Properties of Electricity; in a Letter to the Royal Society from the same.*

Gentlemen,

Read Oct. 30. 1746. 1. **T**HE favourable Reception wherewith you honour'd some Papers I laid before you some time since, relating to Electricity, emboldens me to trouble you again upon the same Subject: And I am the more encouraged so to do, as the Progress of our Discoveries therein, both here and abroad, has been so rapid; that what, little more than a Year ago, we conceived to be the *ne plus ultra* of our Inquiries, is now regarded as mere Rudiments.

2. It were trespassing too much upon you, to recount the great Number of Experiments I have made; for which Reason I shall only take notice of such as are either in themselves striking, or tend to illustrate some Proposition.

3. At the Beginning of last Summer I caused a Machine to be made for electrical Purposes; the Wheel whereof was four Feet in Diameter. In the Periphery of this Wheel were cut four Grooves, corresponding with four Globes of ten Inches Diameter, which were disposed vertically at about three Inches Distance from each other. One, two, or the whole Number of these Globes might be used at Pleasure. They were mounted upon Spindles of two Inches Diameter, and their mean Motion round their Axis was about eleven hundred times in a Minute. As it is next to impossible to have these Globes blown and mounted perfectly true, I order'd the Leather Cushions, with which they were rubb'd, to be stuffed with an elastic Substance (curled Hair) that the Globes in their Rotations might be as equally rubb'd as possible. You might likewise cause the Globes to be rubb'd by the Hands of your Assistants; but under a certain Treatment (of which hereafter) the Cushions excite equally strong. The Leather Cushions were now-and-then rubb'd over with Whiting. As a minute Detail of the Parts of this Machine would take up too much of your Time, I have herewith laid before you a Draught thereof.

4. I lined one of these Globes to a considerable Thickness, with a Mixture of Wax and Resin, in order to observe whether or no the Electricity would be the sooner or more strongly excited; but I found no Difference in the Power of this Globe from the others, which were without this Treatment.

5. The Power of Electricity is increased by the Number and Size of the Globes to a certain Degree;

but by no means in proportion to their Number and Size: Therefore, as the Bodies to be electrified will contain only a certain Quantity of Electricity, of which more largely hereafter; when that Quantity is acquired, which is soonest done by a Number of Globes, the Surcharge is dissipated as fast as it is excited.

6. After the Globes had been a few times used, I found myself Master of a much greater Quantity of electrical Power, with much less Labour to myself, than when I used only Tubes. I could attract and repel light Substances at a much greater Distance than before; fire Spirits of Wine, Camphire, and all other Substances whose Vapours were inflammable, with great Ease, and at any Distance, with Non-electrics placed upon Originally-electrics: I could fire them, I say, at all times; though not equally easy, when the Weather was moist.

7. I discover'd with this Machine, and communicated to several Members of this *Society*, several of the Experiments said to be first made by M. *le Monnier* at *Paris*, before the Letter communicating them was received by our most worthy *President* from thence.

8. I order'd another Machine to be made for a Friend of mine, which carried a Globe of sixteen Inches Diameter. I united the Power of this large Globe with that of three of the others before-mention'd, and found the Strokes from the excited Non-electrics not increased according to my Expectation. In two Experiments indeed, where the Dissipation of the whole Power of these Globes was visible as fast as it was excited, the Effect of this
additional

additional Globe was very considerable. The first was, when two Pewter Plates were held, one in the Hand of an electrified Man, and the other by one standing upon the Floor: When these Plates were brought near each other, the Flashes of perfectly pure and bright Flame were so large, and succeeded each other so fast, that, when the Room was darkened, I could distinctly see the Faces of thirteen People, who stood round the Room. The other was from a Piece of large blunt Wire hanging to the Gun barrel; from the End of which, when electrified, and any black * Non electric unexcited was brought near, though not near enough to cause a Snap, a Brush of blue lambent Flame, totally different from the former, was very conspicuous when the Room was dark, of more than an Inch long and an Inch thick. I mention that what is held near the Bottom of the Wire should be black, because then you see this Flame more sharp. Here the phosphoreal Smell might be perceived at a considerable Distance. If the Back of your Hand was brought so near this Wire as to occasion a Snap, and these Snaps were received for some time, you would feel them

* In the Course of these Observations, whenever I mention either Originally-electrics or Non-electrics, I always understand the whole *Genus* of each. Thus when I mention a Man placed upon Originally-electrics, I am indifferent whether he is suspended either in Lines of dry Silk, Hair, or Wool; or (which is much more convenient) if he stands upon Glass, Wax, Resin, Pitch, Sulphur, &c. or upon different Mixtures of these, if of a sufficient Thickness. As we are now Masters of a greater electrical Power than heretofore, I have found the Electricity pervade, tho' in very small Quantity, Originally-electrics of above four Inches Diameter.

them like so many Punctures upon your Skin, occasioning red Spots, which have lasted four and twenty Hours.

9. If, when a Person is electrified, he brings his Hand upon the Cloaths of one that is not, they both have a Sensation exactly resembling that of many Pins running into the Skin, which continues as long as the Globes are in Motion. This is most perceptible when the Cloaths are of thin woollen Cloth or Silk, animal Substances; less so, when of Linen or Cotton, which are vegetable.

10. If some Oil of Turpentine is set on fire in any Vessel held in the Hand of an electrified Man, the thick Smoke that arises therefrom receiv'd against any Non-electric of a large Surface, held in the Hand of a second Man standing upon an electrical Cake; this Smoke, I say, at a Foot Distance from the Flame, will carry with it a sufficient Quantity of Electricity for the second Man to fire any inflammable Vapour. The electrical Strokes have been likewise perceptible upon the touching the second Man, when the Non-electric held in his Hand has been in the Smoke of the Oil of Turpentine between seven and eight Feet above the Flame. Here we find the Smoke of an Originally-electric a Conductor of Electricity.

11. Likewise if burning Spirit of Wine be substituted in the Place of Oil of Turpentine, and if the End of an iron Rod in the Hand of the second Man be held at the Top of the Flame, this second Man will kindle other warm Spirits held near his Finger. Here we find that Flame conducts the Electricity, and does not perceptibly diminish its Force.

12. These two Experiments demonstrate, that the Opinion of those is erroneous, who suppose the electrical *Effluvia* to be of a sulphurous Nature; and that these themselves are set on fire at the Shaping observ'd, when you bring Non-electrics unexcited to those that are. If their Opinions were true, the electrical *Effluvia* should be destroyed by the Flame in both the preceding Experiments; the contrary of which is observed.

13. I now proceed to take notice of that surprising Effect, that extraordinary Accumulation of the electrical Power in a Phial of Water, first discover'd by Professor *Muschenbroek*, a Man born to penetrate into the deepest Mysteries of Philosophy: And I hope I shall stand excused, if I enter into a minute Detail of the Circumstances relating thereto. The Experiment is, that a Phial of Water is suspended to a Gun-barrel by a Wire let down a few Inches into the Water through the Cork; and this Gun-barrel, suspended in silk Lines, is applied so near an excited glass Globe, that some metallic Fringes inserted into the Gun-barrel touch the Globe in Motion. Under these Circumstances a Man grasps the Phial with one Hand, and touches the Gun-barrel with a Finger of the other. Upon which he receives a violent Shock through both his Arms, especially at his Elbows and Wrists, and across his Breast. This Experiment succeeds best, *cæteris paribus*,

1. When the Air is dry.

2. When the Phial containing the Water is of the thinnest Glass.

3. When the Outside of the Phial is perfectly dry.
4. In proportion to the Number of Points of non-electric Contact. Thus if you hold the Phial only with your Thumb and Finger, the Snap is small; larger when you apply another Finger, and increases in proportion to the Grasp of your whole Hand.
5. When the Water in the Phial is heated; which being then warmer than the circumambient Air, may not occasion the condensing the floating Vapour therein upon the Surface of the Glafs.

14. From these Considerations it is to be observ'd, that this Effect arises from electrifying the non-electric Water, included in the originally-electric Glafs; so that whatever tends to make the Outside of the Glafs non-electric by wetting it, as, a moist Hand, damp Air, or the Water from the Inside of the Phial, defeats the Experiment, by preventing the requisite Accumulation of the electrical Power.

15. That a Gun-barrel is absolutely necessary to make this Experiment succeed, is imaginary; a solid Piece of Metal of any Form is equally useful. Nor have I yet found, that the Stroke is in proportion to the Quantity of electrified Matter; having observed the Stroke from a Sword as violent as that from a Gun-barrel with several excited iron Bars * in Contact with it.

16.

* If of six Men touching each other, and standing upon Originally-electrics, one touches the Gun-barrel, the whole are electrified; all these then must be consider'd, as so much excited non-electric Matter. From the Aggregate of all these, not more Fire is visible upon the Touch than from either of them singly,

16. I have tried the Effect of increasing the Quantity of Water in the Glasses of different Sizes, as high four Gallons, without in the least increasing the Stroke. If * Filings of Iron are substituted in the Room of Water, the Effect is considerably lessen'd. If Mercury, much the same as Water; the Stroke is by no means increased in proportion to their specific Gravities, as might have been imagined §.

17. The Phial should not be less than can conveniently be grasped. I generally make use of those, which hold seven or eight Ounces, and fill them about four Fifths with Water; and the Stroke from one of these, under the same Circumstances, is equally strong with that of a *Florence* Flask held in the Hand, which I have sometimes made use of; though the Glass of this last is equally thin with that of the Phial, and the Quantity of Water four times as much. That the Stroke therefore is not as the Quantity of Water electrified, is evident from this Experiment. This Fact does not depend upon my Judgment alone, but likewise upon the Opinions of several learned Members of this *Society*, who have experienced the greater and less Quantity of Water.

18. If a dry Twig of Birch, or any other Wood, be run through the Cork instead of the metallic Wire,

* For a further Account of the Filings of Iron, made use of in this Experiment, see these *Transactions*, Vol. XLV. p. 197.

§ In this Experiment, and in others, wherein we assert, that the Stroke is not increased in proportion to the Quantity of electrified Matter; it must always be understood, that the excited Non-electrics themselves are touched, without being contained in Originally electrics, as Water in the Glass; for otherwise (as will hereafter be specified) the Effects of different Quantities of Matter will be very different.

Wire, the Stroke is not greater than is usually felt from the Gun-barrel without the Application of the Water. The Stroke is likewise lessen'd, if the Phial is held in the Hand with a Glove on.

19. After the Gun-barrel and Phial have been sufficiently excited, which is done in a few Seconds, the Surcharge is dissipated; so that the continuing the Motion of the Machine ever so long after the Saturation is complete, does not increase the electrical Force.

20. The Force of the Stroke from the electrified Phial does not increase in proportion to the Dimensions of the Glass, or the Number of Globes employed. I have been struck as forcibly with one Phial from a Globe of seven Inches Diameter, as when I made use of, at the same time, one of sixteen Inches, and three of ten. I have been lately informed, that at *Hamburg* a Sphere was employed for this Purpose a *Flemish* Ell in Diameter, without the expected Increase of Power.

21. When the Phial is well electrified, and you apply your Hand thereto, you see the Fire flashes from the Outside of the Glass wherever you touch it, and crackles in your Hand.

22. The Phial may be electrified by applying the Wire therein to the Globe in Motion; after which, if it is grasped in one Hand, and the Wire touched with a Finger of the other, the Stroke is as great as from the Gun-barrel. If you only bring your Finger near the End of the Wire without touching it, you observe the same Brush of blue Flame, as from the Wire hanging to the Gun-barrel, before taken notice of. This instantly disappears upon touching

touching the Wire, though you do not receive a Shock, unless at the same time you grasp the Phial.

23. If you grasp the Phial with your Hand, and do not at the same time touch the Wire, the acquired Electricity of the Water is not diminished. So that, unless by Accident or otherwise the Wire is touched, the electrified Water will contain its Force many Hours, may be convey'd several Miles, and afterwards exert its Force upon touching the Wire.

24. If, when the Machine is in Motion, the Phial is hung upon the Gun-barrel, no Increase of the Stroke is perceived upon touching the Gun-barrel with your Finger, unless at the same time the Phial is taken in the Hand.

25. If, when the Gun-barrel and Phial are excited, you grasp the Phial with one Hand, and touch the Gun-barrel with a Piece of any Metal held in the other, the Shock is as great in your Arms as though you touched the Gun-barrel with your Finger; but not the least Shock is felt, if, instead of Metal, you touch the Gun-barrel with a Piece of dry Wood.

26. I have felt a very great Stroke, when I hung two Phials to the Gun-barrel, and, grasping them both, brought my Forehead near it. The Shock then was so violent, that I seem'd stunn'd, as though struck on the Head with a great Stick, and I have never since chose to repeat this Experiment. This Increase of the electrical Force was owing to the additional Phial, whereby the Points of non-electric Contact were augmented.

27. Likewise if a Person placed upon Originally-electrics, grasps two Phials, as before-mention'd, and a second Person, standing upon the Floor, touches any Part of his Body, a very slight Stroke only is perceived. But if the second Person, while the Globes are in Motion, places one of his Fingers upon the Hand, or any Part of the naked Body of the first, and at the same time touches the Gun-barrel with his other Hand; both feel a Shock equal to that just now mention'd, but more tolerable, because not felt in the Head, in the Arms only, and across the Breast. In this Experiment, it is not necessary that the Outside of the Glasses held in the Hands should be dry, as in the former Experiments; because whatever by the Moisture is communicated to the Man, is stopped by the Originally-electrics upon which he is placed. If, instead of his Hand, you gently touch the first Person's Cloaths, you only perceive a small Stroke upon your Finger; but if you press his Cloaths close to his Body, you frequently perceive a double Stroke; the one, slight from his Cloaths; the second, a violent Shock from his Body.

28. Upon shewing some Experiments to Dr. *Bevis*, to prove my Assertion that the Stroke was, *æqualis paribus*, as the Points of Contact of Non-electrics to the Glass, that ingenious Gentleman has very clearly demonstrated it likewise by the following Experiment: He wrapped up two large round-bellied Phials in very thin Lead so close as to touch the Glasses every-where, except their Necks. These were filled with Water, and cork'd, with a Staple of small Wire running through each Cork into the Water.

Water. A Piece of strong Wire about 5 Inches long, with an Eye at each End, was provided, and at each End of this hung one of the Phials of Water by the small Staple running through the Cork. A small Wire Loop then was fasten'd into the Lead at the Bottom of each Phial, and into these Loops was inserted a Piece of strong Wire like the former. If then these Phials were hung across the Gun-barrel and electrified, and a Person standing upon the Floor touched the bottom Wire with one Hand, and the Gun-barrel with the other, he received a most violent Shock through both his Arms, and across his Breast.

29. These Phials may be concealed, and the Shock be more universal, in the following manner: The Phials may be placed in a Corner of the Room, and any thing laid over them, so as not to touch the upper Wires; then a very fine Wire must be suspended to the Gun-barrel, and fasten'd to the upper strong Wire. A second Piece of small Wire, of a sufficient Length to reach from the Phials almost under the Gun-barrel, must be fastened to the lower strong Wire, and this may be conceal'd under a Floor-cloth. The Phials then are electrified; and if a Person, placing his Foot upon the Floor-cloth over the Wire which comes from the Bottom of the Phials, touches the Gun barrel, he receives a most terrible Shock. The first time I experienced it, was when the Phials were fully electrified, and both my Feet were placed upon the Wire. Upon receiving the Stroke from the Gun-barrel upon my Finger, it seem'd to me, used as I am to these Trials, as though my Arm were struck off at my Shoulder,

Shoulder, Elbow, and Wrist; and both my Legs, at the Knees, and behind near the Ankles. So that, to try the Effects of this Experiment, you must be careful of not electrifying the Phials too much. If a dozen or more of these Phials, or one very large Bottle, were cover'd over with thin Lead in the above manner, and strongly electrified, and this Electricity were discharged by a Man at once in the manner here mention'd, I should dread the Consequences.

30. We must observe, that this Shock is not felt, unless the Wire, coming from the Bottoms of the Bottles, is touched; and then not, if the Shoes are dry, and of consequence originally-electric. In this Experiment we see the Effects of the Increase of the Points of Contact; and it seems the more surprising to those who are not acquainted with the Cause, when the Wire is concealed under a Floor-cloth, that the moving of their Feet only one Inch, should occasion them, all other Circumstances apparently the same, to feel a violent Shock, or none at all. A thick Carpet, instead of a Floor-cloth, is liable to prevent the Success of this Experiment, for the same Reason as dry Shoes. This Experiment may aptly enough be called, the springing an electrical Mine.

31. If, in the former Experiment, the lower small Wire is fasten'd to an iron Rod; and if, when the Phials are ever so strongly excited, that Rod is held in the Hand of a Man standing upon the Floor, and with it he touches the Gun-barrel, he perceives no Shock, for Reasons presently to be assigned. But if he takes this iron Rod in one Hand, and touches the Gun-barrel with the other, he then is violently struck.

struck. We must here observe, that the Violence of the Stroke is always felt in our Bodies, in proportion to the Loudness of the Explosion, and the Quantity of Fire seen. Therefore, as both these are equally perceptible, whether the Electricity passes only thro' the Iron, as in the first of these Instances, or thro' our Bodies equally with the Iron, as in the second; we conclude, that in both there is in the same Degree of electrical Force. By the first of these Methods you are capable of making others sensible of the electrical Force, without feeling it yourself. This Experiment, as well as the last, will admit of infinite Variation.

32. If a Man, standing upon an electrical Cake, takes the Phial suspended to the Gun-barrel in his Hand, by these means he acquires some electrical Power; for if, under these Circumstances, he touches the Gun-barrel, he only receives a slight Stroke. If then, without having had any Communication with unexcited Non-electrics, he touches the Gun-barrel again, the Globes being yet in Motion, he receives no Stroke at all.

33. If to the Gun-barrel an Egg, either raw or boiled, is suspended by a Piece of Wire, and a Person, grasping the electrified Phial in one Hand, brings the Palm of his other near the Bottom of the Egg; at that Instant he receives a smart Stroke, and his Hand seems full of a more red Fire than is usually observed. In this Experiment the Stroke is more confined to the Hand without shocking the Arms, than when you touch the Gun barrel itself; it more resembles a Stroke over the Hand with a *Ferula*.

34. If any Number of People stand upon Originally-electrics, and communicate with each other by any non-electric Medium, especially Metal, they are by these means all equally electrified; and if a Person standing upon the Floor, and holding the Phial of Water hanging to the Gun-barrel in his Hand, touches the Person furthest from the Gun-barrel, the whole Number receives a Shock equal to any one touching the Gun-barrel singly.

35. If a Number of Persons, how great soever, stand upon the Ground, communicating with each other as before, the first of which grasps the Phial, and the last touches the Gun-barrel, the whole Number receive a Shock like the former. This, we are inform'd, M. le Monnier at Paris communicated through a Line of Men, and other Non-electrics, measuring nine hundred Toises.

36. Several Experiments shew, that the electrical Force always describes a Circuit; e.g. if a Man holds the electrified Phial in one Hand, and touches the Gun-barrel with the other, he feels the Shock in no other Parts of his Body than in his Arms, and across his Breast. So that here we see the electrical Power darts *rectissimo cursu* between the Gun-barrel and Phial. This is more particularly demonstrated by the following Experiment, in which, though the two Lines of Persons may be of any Length, we only specify, that each consists of four, for the sake of Perspicuity.

37. Of one Line, let A (see TAB. III.) touch the Gun-barrel, standing upon Wax, and communicate with BCD likewise standing upon Wax. Of the other Line, let 1 take the electrified Phial in his Hand,

Hand, and join with 2, 3, and 4, all standing upon the Floor. If, under these Circumstances, the first Line is electrified, and 4 touches *D*, all eight are struck through. If 4 touches *C*, *D*, though electrified, feels nothing, and the remaining seven are struck; so that here *D* is left out of the Circuit. If 4 touches *B*, only six feel the Shock, and *C* and *D* feel nothing; and thus you may proceed to *A*, who must always necessarily feel, if either himself or any of his Line is touched. If, when both Lines are as before-mention'd, *D* touches 3, 4 is left out of the Circuit, and the remaining seven feel the Stroke. If *C* touches 2, the Circuit consists of five, *D*, 3, and 4 being, though under the same Circumstances, left out: Always observing, however these Circuits are diversified, that *A*, who touches the Gun-barrel, and 1, who holds the Phial, are certain to feel the Stroke.

38. This Experiment may be reversed, the Lines being as before, in the following manner, wherein likewise this Circuit is always observable. Let *A* (see TAB. III.) touch the Gun-barrel as before, and *D* hold the Wire of the electrified Phial in his Finger. Let 4 grasp the Phial, and 1 touch *B*; then *A* feels nothing, being left out of the Circuit, and the other seven are struck. If 4 touches *C*, then *A* and *B* feel nothing, the Circuit consisting of the remaining six. But it is to be observed, as in the former Experiment, that 4, who grasps the Phial, and *D*, who holds the Wire, must of Necessity be always in the Circuit. I have been the more particular in this Matter, as it demonstrates the Course of

the electrical Power to be in the most direct manner between the Gun-barrel and the electrified Phial.

39. Likewise, if a Person, standing upon an Originally-electric, touches the Gun-barrel with his right Hand, a Piece of Wire being placed round his left Leg, and a second Person, standing likewise upon the Wax, takes hold of the Extremity of this Wire; then let another Person, standing upon the Floor, and grasping the electrified Phial, touch any Part of the second Person's Body. Upon this Touch, the second Person is shook as usual; but the first feels the Stroke only in his left Leg and right Arm, the nearest Course of the electrical Power.

40. If any Number of Persons communicate by Pieces of Wire, and if any one of them brings together the Ends of the two Pieces of Wire in his Hands, upon the Gun-barrel's being touch'd, he will perceive no Stroke. But if the Ends of the Wires are but a Quarter of an Inch asunder, he will be shook in both his Arms; because then his Body will become Part of the Circuit.

41. If, when any Number of Persons join Hands, or communicate by any metallic Medium standing on the Floor, one grasps the Phial, and joins with the rest; upon the Gun-barrel's being touch'd by the last Person of the Line, the whole Number are struck; and he who grasps the Phial, as forcibly as the rest. But if two Phials are employed, and he grasps them both, with a Piece of Wire of sufficient Length held between his Fingers, which Wire touches both Phials, and its End is taken hold of by the second Person of the Line; if then the last Person touches the excited Gun-barrel, all in the
Line

Line are violently struck, except the Person who grasps the Phials ; but he feels little or nothing of the Stroke.

42. The Stroke is very violent, when a Wire is put round the naked Head, or under the Peruke, and the Person grasping the Phial touches the Gun-barrel with the Ends of the Wire, or if he holds the Wire between his Teeth.

43. If a Person, standing on the electrical Cakes with Gold or Silver Lace upon his Coat, takes hold of the Gun-barrel, and another Person grasping the electrified Phial touches the Bottom of the Lace, the Person electrified, if he holds down his Head, feels the Blow under his Chin. The Lace in this Instance has the same Effects as a Piece of Metal; at the End of which, if placed in the same manner, you would necessarily feel the Stroke.

44. I now proceed to shew, by what Steps, in my Inquiries into the Nature of Electricity, I discover'd that the glass Tubes and Globes had not the electrical Power in themselves, but only served as the first Movers and Determiners of that Power.

45. Several Months since, I observ'd that, by rubbing a glass Tube, while standing upon a Cake of Wax, in order, as I expected, to prevent any of the electrical Power from discharging itself through me into the Floor; contrary to my Expectation, that Power was so much lessen'd, that no Snapping was to be observ'd upon another's touching any Part of my Body. But if a Person not electrified held his Hand near the Tube whilst it was rubbing, the Snapping was very sensible. This I shew'd to several Members of the *Royal Society*,

and other, who did me the Honour to visit me. Afterwards I met with an Experiment of the same kind, in a Treatise publish'd by Professor *Bose*, intitled, *Recherches sur la cause et sur la véritable theorie de l'Electricite*, which that ingenious Gentleman says, had given him great Trouble by its Oddness. The Experiment is, that, if the electrical Machine is placed upon Originally electrics, the Man who rubs the Globes with his Hands, even under these apparently favourable Circumstances, gives no Sign of being electrified, when touched by an unexcited Non-electric. But if another Person, standing upon the Floor, does but touch the Globe in Motion with the End of one of his Fingers, or any other Non electric, the Person rubbing is instantly electrified, and that very strongly. The Solution of this *Phænomenon*, seemingly contrary to the already discover'd Laws of Electricity, had terribly tormented him; but however he has given us the following, which he modestly calls a plausible Subterfuge rather than a Solution; *viz.* that a Power cannot act at the same time with all its Vigour, when one Part of it is already employed; as a Horse, who already draws an hundred Pounds, cannot draw an additional Weight as freely as if he had not been loaded at all. That the Hand excites the Virtue already in the Sphere; therefore if the same Power impregnates the Man, there remains none for the Globe. That the Virtue of the Globe then cannot be communicated at the same time to the Man, by whom it is created. That he, who gives it, cannot receive it himself. From these, and such-like Considerations, it appears to him,

him, that the Man upon the Ground, who holds his Fingers to the Globe in Motion, instead of his diminishing its electrical Force, throws that Force back again over the Man, who excited it. That the Finger in this Case seems to operate as an Electric *per se*, and drives back the electrical Power.

46. I have seen an Account of * Mr. *Allamand*, lately printed at the *Hague*; wherein he takes notice of this *Phenomenon*. He tells us, that as Part of the electrical Power of the Globe passes off by the Frame, upon which the Globes are mounted, into the Floor, and dissipated thereby; he conceived, that if the Machine, and the Man who rubb'd the Globe, were placed upon Pitch, to prevent this Dissipation, the Fire of Electricity would be more strong. But the Consequence is extremely odd and unexpected; for the contrary happens; and the electrical Power is considerably diminished, and sometimes there is even none at all.

47. I tried this Experiment several times with my Machine, and the Man, who turns the Wheel thereof, mounted upon the electrical Cakes. If the Air was dry, and the Machine placed at some Distance from non-electrical Substances, as the Sides of the Room, Chairs, and such-like; after one or two small Snaps, the Gun-barrel, supported by silk Lines, and hanging in Contact with the Globes, would, tho' the Machine were in Motion a considerable time,
attract

* *Bibliothèque Britannique pour les Mois de Janvier, Février, et Mars, 1747.*

attract no light Substances, nor emit any Fire. This induced me to conceive, that the electrical Power was not inherent in the Glass, but came from the Floor of the Room; and if the Fact were so, the Gun-barrel should snap upon my touching any Part of the Machine. The Consequence fully answer'd my Conjectures; for while I stood upon the Floor, the Globes still in Motion, I put one Hand upon the Frame of the Machine, and touched the Gun-barrel with one of the Fingers of my other. Upon this, Fire issued, and the Snapping continued as long as I held my Hand upon the Machine, but ceased upon taking it off. This at once proved to me, that the electrical Fire passed from the Floor thro' my Body to the Machine. I then order'd the Man to put one of his Feet from the Wax upon the Floor; which, as soon as he complied with, caused the Electricity to snap at the Gun-barrel, and this ceased upon his replacing his Foot. Here I found, that the electrical Power came through the Man; and that, in these Instances, either myself, or the Man who touched the Floor with his Foot, was to be regarded as an additional Part of the Machine communicating with the Floor. These Considerations led me to make the following Experiments.

~~18.~~ If my Conjectures were well founded, and if ~~the electrical Power~~, the Man and the Machine being placed upon Originally-electrics, went through my Body to the Machine, a fine Wire, held in my Hand at a few Inches Distance, ought to be attracted by any Part of the Machine. This succeeded accordingly, but the Attraction lasted a very small Space

Space of Time, and the Wire again hung perpendicularly from my Finger, though the Globes continued in Motion. This induced me to believe, that the Gun-barrel, and the other Non-electrics suspended in Contact with the Globes, would only contain a certain Quantity of the electrical *Æther*; and if this were the Case, the Attraction of the Wire to the Machine would be continual, if the electrical Power found again a Communication with the Floor, as the Wire was the only Canal of Communication between the Floor and the Machine. Whereupon I placed one of my Fingers upon the Gun-barrel, and held a Wire near the Machine with my other Hand, and found, that as long as my Finger continued upon the Gun-barrel, the Wire was attracted, but no longer.

49. Here we find, that one Cause of the electrical Attraction is the Current of the electrical *Æther* setting to the Machine through the Wire; and this Current is stopped from two Causes; one, when there is no Discharge thereof from the Gun-barrel, the Accumulation being complete; the other, when other Currents are opened, that is, when the Machine is touched in other Parts.

50. In these, and the subsequent Experiments, I always suppose the Air very dry; for if it is not, and the silk Lines, which support the Non-electrics, are wetted thereby, the electrical Power will be discharged along them, and the Wire will be constantly attracted, as I have frequently on purpose experienced; and this Discharge is in proportion as the Lines are more or less wetted.

51. If a Man stands upon the Machine placed upon Originally-electrics, and the Gun-barrel with the other Non-electrics are suspended as usual in Contact with the Globes. no Electricity is observed in that Man: But if a Wire, hanging to the Wainscot of the Room, touches the Gun-barrel, or a Man standing upon the Floor applies his Finger thereto, the Man upon the Machine emits Fire copiously; and either himself, or the Man who turns the Wheel of the Machine, fires inflammable Substances. But this Effect is no longer observable, when the Wire, &c. are removed from touching the Gun-barrel. So that, in this Experiment, the usual Course of the Electricity is inverted; and that Power, which, in most other Instances, is brought by the Wood-work of the Machine to the Globes, and by them discharged upon the Gun-barrel, is now brought by the Wire to the Gun-barrel, and from this the Globes throw it all over, not only the Machine, but any Non-electric in Contact with it, if the Electricity is stopp'd. In this Experiment, if an iron Rod, standing upon the Floor, is inclined against the Loops of the silk Lines which support the Gun-barrel, in such a manner as not to touch the Gun-barrel, the electrical Fire, which passes from the iron Rod to the Gun-barrel, instead of being supplied constantly, comes in by snapping so long as any unexcited Non-electric communicates with the Machine, but ceases upon its being removed: And if the Air is very dry, and none of the Electricity conducted down the silk Lines, the Snapping from the iron Rod to the Gun-barrel will frequently correspond to the touching of the wooden Machine with
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your Fingers, and stop upon your taking them off. And this Experiment will look much like Magic, even to those who are acquainted with the Operations of Electricity; for if the Person who turns the Wheel of the Machine, and stands upon the Cakes, be properly instructed: upon your bidding the Gun-barrel snap, he only puts the Toe of his Shoe upon the Floor, and it snaps immediately, and continues snapping as long as he keeps it there; but if you order it to cease snapping, he almost imperceptibly replaces his Foot upon the Cakes, and it ceases. This may be repeated as often and as long as you please.

52. Many Experiments demonstrate, that if the Electricity is not stopt, no Sign of its Presence, either by Fire or Attraction, is observable in the non electric Bodies suspended to the Globes: that is, although ever so great a Quantity be determined by the Globes over these Bodies, the Electricity passes off from them *pleno rivo* to the Floor, from whence it came: but if the Electricity is stopt, it is then accumulated upon these Non-electrics; but this can be done only to a certain Degree, as is manifest from a former Experiment. And if, when this Power is accumulated, a Man standing upon the Floor touches now-and-then the Non-electrics with his Finger, the Electricity, which is here accumulated, snaps, and the Fire is always observable. But this Snapping is not, when the electrical Power passes off continually, as from a Piece of blunt Wire hung to the suspended Gun-barrel, and the Hand of a Man brought near it without touching; whereby the electrical Power becomes visible, like a fine

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blue Cone of Flame, with its Point towards the Wire. When the Hand is placed at a proper Distance, the Blast, like that of cold Air, is therefrom very manifest. If you do not determine the Electricity by these means to a Point, the Dissipation of it is general, and from all Parts of the excited Non-electric; but if you do, by bringing your Hand near the Wire as before-mention'd, you see the Manner of its being discharged into the Floor, and so into the Earth. These Facts being so, if my Conceptions are true, that the glass Globes circulate the electrical Fire, which they receive from their Friction against the Cushions, or the Hand of a Man, and which is constantly supplied to these last from the Floor; the Ingress of the electrical Fire, if the Machine, &c. are placed upon Electrics *per se*, ought to be visible, as well as the Egress under the same Circumstances; and this is demonstrated by Experiment. For if, while any unexcited Non-electrics touch the Gun-barrel, the Globes being in Motion, you bring your Finger, or a Piece of Wire near any Part of the Wood-Work of the Machine, but more especially the iron Axis of the Wheel; you observe the Burst of blue Flame set in from it to the Wood-Work. We always observe, in this Experiment, that the lambent Flame from the End of the Wire passes diverging into the Machine, and this continues so long as the Gun-barrel is touch'd. So that here the Office of the Globes exactly tallies with that of the Heart in Animals; which, as long as the Quantity of Blood is supplied, propels it into the Arteries, and thence all over the System; or that of the Pump in Hydrostatics. In the same manner,

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by the Attrition of glass Tubes, the electrical Power is brought from the Body of the Man who rubs the Tube ; and he is constantly taking in a Supply from the Floor.

53. What I here call the electrical *Æther*, is that Atmosphere which surrounds both excited Originally electrics, and excited Non-electrics. That this is extended to a considerable Distance, appears, from a fine Thread, or Piece of Cotton-grass Seed, being attracted at some Distance from them, as far as which, it is presumed, this Atmosphere extends. Here indeed it is only perceived by its Effects upon these light Substances : but at the Brush of Flame from the End of the Wire before-mention'd, from some Bran lying upon a flat Piece of Metal in Contact with excited Non-electrics, your Hand being held over it, and in many other Experiments, it becomes manifest to your Feeling as a Blast of cold Wind. You feel it likewise in a less Degree, when a glass Tube is well excited, and brought near your Face. If no unexcited Non-electric is near, this Atmosphere seems to be determined equally over all the excited Non-electrics in Contact with the Machine ; but if a Non-electric unexcited is brought near, the greatest Part of it is determin'd that way ; and hereby the Attraction at any other Part of these excited Non-electrics is considerably diminished. Hence the Cause of the Repulsion of Electricity, which does not operate, until the electrical *Æther* is sufficiently accumulated. This electrical Repulsion is strongest in those Parts of the excited Non-electrics, where unexcited Non-electrics are brought near them ; for by these the electrical Blast, which

otherwise is general, is particularly determined to the Floor.

54. Before I proceed further, I must beg Leave to explain what I call the Accumulation of Electricity. To put a similar Case: As we take it for granted, that there is always a determinate Quantity of Atmosphere surrounding the terraqueous Globe, we conceive, when we see the Mercury in the Barometer very low, that there then is a less accumulated Column of this Atmosphere impending over us, than when we see the Mercury high. In like manner when we observe that the electrified Gun-barrel attracts or repels only very light Substances at a very small Distance, or that the Snap and Fire therefrom are scarcely perceptible; we conceive then a much less Quantity of electrical Atmosphere surrounding the Gun-barrel. This Power being more or less, we call the greater or less Degree of the Accumulation of Electricity. This is only attainable to a certain Point, if you electrify ever so long; after which, unless otherwise directed, the Dissipation thereof is general. The Phial of Water of *Muschenbroek* seems capable of a greater Degree of Accumulation of Electricity, than any thing we are at present acquainted with: And we see, when, by holding the Wire thereof to the Globe in Motion, the Accumulation being complete, that the Surcharge runs off from the Point of the Wire, as a Brush of blue Flame. A Method has been discover'd here by a Gentleman (Mr. *Canton*) by which the Quantity of accumulated Electricity may be measured to great Exactness. The Manner of measuring is this: When the Phial is sufficiently electrified by
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applying the Wire thereof to the glass Globe, and which is known by the Appearance of the Brush of Flame at the End of the Wire, as before-mention'd; hang a slender Piece of Wire to the suspended Gun-barrel for this Purpose detached from the Globes. Upon your applying the Wire of the electrified Phial to that hanging to the Gun-barrel, you perceive a small Snap; thus you discharge by touching the Gun-barrel with your Finger, which likewise snaps: And thus alternately electrifying and discharging, you proceed until the whole Electricity of the Water is dissipated; which sometimes is not done, under an hundred Discharges. If you do not discharge the Electricity every time, the Snaps from the Wire of the electrified Phial to the Gun-barrel are scarcely perceptible. In proportion to the Number of Strokes, you estimate the Quantity of the acquired Electricity of the Water. That you could, by stopping the Electricity, excite Non-electrics; and, by accumulating their Power, make them exert more Force than Originally-electrics would at any Point of Time, was that capital Discovery of the late Mr. Gray; and is to be regarded as the Basis, upon which all the present Improvements of our Knowledge in Electricity are founded; and till which Discovery, although some of the Effects of Electricity were observed above two thousand Years ago*, little Progress was made.

55.

* *Theophrastus*, who lived above three hundred Years before the Date of the Christian *Æra*, takes Notice of Amber and the *Lyncurium*,

55. The electrical *Æther* is much more subtil than common Air, and passes to a certain Depth through all known Bodies. It passes most readily through Metals, Water, and all Fluids, except resinous ones; then animal Bodies dead or alive, in proportion as they are more or less wet; then Stones, Wood, and Earths. It passes to a certain Thickness only thro' Resins, dry animal Substances, Wax, and Glass. For this Reason Bodies are called Electrics *per se*, or Non-electrics; not only for their rubbing the Electricity from other Bodies, but likewise as they permit more or less of the electrical *Æther* to pass through them. This *Æther* has not only the Property with Air of moving light Substances; but it seems to have another, and that is Elasticity.

56. That this Fluid is more subtil than common Air, is more particularly demonstrated by its passing through several Glasses at the same time; through any one of which, though ever so thin, Air cannot pass. It likewise passes, as I have mention'd before, through all known Bodies, except Originally-electrics, and even through these to a certain Degree. Its Elasticity is proved by its extending itself round excited Electrics, and excited Non electrics, to a considerable Distance; as well as by its increasing the Motion of Fluids. This is demonstrated by the Experiment with a small glass Siphon where
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curium attracting not only Straws, and Shavings of Wood, but also thin Pieces of Copper and Iron. See *Theophrastus* *ἐπὶ τῶν λίθων* v.

—Καὶ τὰ λυγρὰ — αἱ καὶ τὰ σίδηρον, καὶ χαλκόν. Οἱ δὲ σιδηροὶ καὶ χαλκοὶ καὶ ξύλον, ἀλλὰ χαλκὸν καὶ σίδηρον, εἰς ἣν λυγρὰ. *Ἰσοπερὶ καὶ Διοκλῆς* ἐπεὶ γὰρ. See p. 74. in the late Edit. by J. Hill.

the Elasticity of the electrical *Æther* overcomes the Attraction of Cohesion: I have frequently observed this Experiment does not operate, unless the greatest Part, if not the whole electrical Blast, is determined to the Floor through the Water, by bringing some unexcited Non-electric near the long Leg of the Siphon †. The Stream through this slender Tube is most complete, when the Non-electric is brought near, so as when the Room is somewhat darkened, the Stream of Water appears as a Stream of blue Flame, much like that from the blunt Wire. This Stream is stopped, either by touching any Part of the Non-electrics in Contact with the Globes; by placing the Machine and the Man who turns the Wheel upon Electrics *per se*, by which the Current of the electrical *Æther* from the Floor to the Machine is prevented; or by removing the Non-electric from the Leg of the Siphon, by which the Dissipation of the electrical *Æther* from the excited Non-electric becomes general. So that we find, that although we can repel light Bodies from many Parts of excited Non-electrics at the same time; the whole Force of the electrical Current is necessary, to drive
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† There is one Instance, where the Water will run off in a full Stream without bringing a non-electric unexcited near the long Leg of the Siphon; and that is, by suspending a Phial of Water, as usual to the Gun-barrel by a Wire, and by letting a glass Siphon through the Cork into the Water. When this Phial is sufficiently electrified, the Water therein runs off in a full Stream, though no Non-electric unexcited is near; because then the Current of Water through the Siphon is the only Way, by which the Surcharge of the Electricity can be dissipated.

off so ponderous a Fluid as Water. May we likewise not infer the Elasticity of electrical Æther, from the Ingress of the blue Flame from the End of a blunt Wire held near the Axis of the Wheel, or any Part of the Wood work of the Machine, after the Revolutions of the Globes are ceased? Certainly we see an Influx of electrical Fire to all Bodies, until their determined Quantity is restored. Is not the Elasticity of this Æther deducible likewise from the violent Shock we feel in our Bodies in the Experiments with Water?

57. There seems to be a Quantity of this Æther in all Bodies. Hence the Reason why, though the Machine is placed upon Electrics *per se*, a Snap or two, as I mention'd before, is observ'd upon touching the Gun-barrel, when the Machine has been some time in Motion: But after these no more is perceiv'd, if the silk Lines are very dry, and the electrical Supporters of the Machine are of a requisite Thickness. As soon as any Non-electric unexcited touches the Machine, this Loss is immediately restored. As the electrical Æther, as has been specified, is an elastic Fluid, wherever there is an Accumulation thereof, there is an Endeavour by the nearest unexcited Non-electric to restore the *Æquilibrium*. The restoring of this *Æquilibrium* I take to be the Cause of the Attraction of excited glass Tubes and Globes, as well as that of excited Non-electrics; for here the Blast of electrical Æther constantly sets in from the nearest unexcited Non-electrics towards those excited, and carries with it whatever light Bodies lie in its Course. This setting in
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of the Current of electrical *Æther* towards excited Non-electrics is likewise very perceptible to your Feeling as a Blast of cold Wind ; if when you are electrified, you hold your Hand over a Plate with some Bran in it, by which Blast the Bran is carried against your Hand. These light Substances are again repell'd by the Blast from the excited Bodies, as soon as they come in Contact, and sometimes before. The Successions of these alternate Attractions and Repulsions are extremely quick, so that sometimes your Eye can hardly keep Pace with them. And if you put a glass Globe of about an Inch in Diameter very light and finely blown into a Plate of Metal, and hang another Plate over it ; electrify the upper one, and bring the other under it, and you will find the Strokes from the alternate Attractions and Repulsions * almost too quick for your Ear. I have seen a *German*, who travell'd with a small electrifying Machine, who, by a Process of this sort, made two small Bells ring. One of the Bells was suspended to an electrified Wire, which was conducted without touching along the Sides of the Room ; at about an Inch Distance, detached from this Wire, a little Clapper was hung by a silk Line ; at an equal Distance from this last was hung another little Bell, which communicated with the Sides of the Room.

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* The following is an Argument of the Velocity likewise, with which these little Globes are attracted and repell'd. If they are let fall from the Height of six Feet or more upon a wooden Floor, or a Plate of Metal, they are rarely broke ; but by the Attractions and Repulsions of them between the Plates, though at the Distance only of one sixth of an Inch, they are frequently beat in Pieces.

As soon as the Machine was in Motion, the electrified Bell^s attracted the Clapper, which immediately by the repulsive Blast was blown off to the unexcited Bell. By the time the second Bell was struck, the former attracted again; and this Jingling of the two Bells continued not only during the Motion of the Machine, but several Seconds after it was stopped. This was occasioned by the small Volume of the Clapper being able to convey away only a small Quantity of the electrical *Æther* at each Stroke; by which it was some time before the *Equilibrium* was restored.

58. To demonstrate likewise, that the restoring this *Equilibrium* is not imaginary, I shall mention an Experiment of a Gentleman (Mr. *Wilson*) who has taken great Pains in these Inquiries. Take two Plates of any Metal, very clean and dry, whose Surfaces are nearly equal; hang one of them to any excited Non-electric, and bring under it upon the other a whole Leaf of Silver. When, which you find upon Application, the silver Leaf is attracted, lower the bottom Plate; if it is too low, you will observe the leaf Silver jump up and down; if too high, it will only be attracted in Part, and thereby dissipate the electrical Power. But if you get it at the proper Distance, which will very easily be found upon Trial, the Silver will be perfectly suspended at right Angles with their Planes, like the *Trapezium* of the Geometers, and touch neither of the Plates; it will be extended likewise to its utmost Dimensions. You frequently observe, both at the Top and Bottom of the Silver, the electrical Fire. The same Effect is produced, if you reverse the Experiment,

ment, by electrifying the bottom Plate, and suspending the other over it. Now I conceive, that the Space occupied by this Leaf of Silver, is that where the *Equilibrium* of the electrical *Æther* is restored; for if you take away the under Plate, thro' which from the Floor the Flux of this *Æther* is furnished, or if that Plate be placed upon an Electric *per se*, by which this Flux is prevented likewise, the silver Leaf is blown away.

59. No Body can be suspended in *Equilibrium* but from the joint Action of two different Directions of Power: So here, the Blast of electrical *Æther* from the excited Plate blows the Silver towards the Plate unexcited. This last, in its Turn, by the Blast of electrical *Æther* from the Floor setting through it, drives the Silver towards the Plate electrified. We find from hence likewise, that the Draught of electrical *Æther* from the Floor, is always in proportion to the Quantity thrown by the Globes over the Gun-barrel; or the *Equilibrium* by which the Silver is suspended, could not be maintained. I once found, that a Gentleman, at that time an Invalid, whose Shoes were perfectly dry, and of consequence Originally-electrics, and who was employ'd to hold the Non-electric Plate through which the *Æther* was to come from the Floor; this Gentleman, I say, did not furnish a sufficient Quantity, because of the Dryness of his Shoes, to maintain the *Equilibrium*; and the Silver was blown away. But upon employing another to this Office, whose Shoes were more wet, the *Æther* came readily through him, and the Silver was suspended. I have likewise found a wooden Pole, very dry, not

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conduct this *Æther* fast enough to keep the Silver suspended. It may be imagined, that it is possible for the Silver to be suspended, without supposing a Flux of the electrical *Æther* from the nearest unexcited Non-electric, as well as from the excited one ; that is, by the simple electrical Attraction. But to obviate this, it must be remembered, that the electrified Gun-barrel both attracts and repels light Substances at the same time. Can this Attraction and Repulsion be conceived without the Operation of the electrical *Æther* both to and from the Gun-barrel at the same time? Does not this point out an Afflux as well as an Efflux? Are not the electrical Repulsions as strong at least as the Attractions? Do not we see light Bodies, either between excited Originally-electrics, or excited Non-electrics, and unexcited Non electrics, dart like a Ball between two Rackets of equal Force? It may be said perhaps,

1. That the suspended Silver may only serve as a Canal of Communication, which discharges the Electricity from the excited Non-electric to the unexcited one ; and that when an Originally-electric is placed between the lower Plate in this Experiment and the Floor of the Room, that then the Silver is attracted only, until the lower Plate is saturated with Electricity, and no longer. This is as much as saying that this Effect arises from Electricity, without mentioning in what manner.

2. That this Effect is produced by the electrical Attraction, which gives the Silver a Direction towards the excited Non electric, but that it is kept down near the unexcited one by the Force of Gravity.

vity. Was this the Cause, the Action of Gravity would operate as much thro' Originally-electrics as through Non-electrics.

60. But I am able to prove the Afflux experimentally, as well as the Efflux, in the following manner. When the Silver lies still, though the Motion of the Globes is continued between the two Plates, one suspended to the Gun-barrel, and the other placed upon an electrical Cake, a Person standing upon the Floor needs only bring a small glass Siphon in a Vessel of Water, and apply the long Leg thereof near the Plate placed upon the Wax; for upon this the Silver is immediately suspended; and the Water, which before only dropp'd, now runs in a full Stream, and appears luminous*. Does not, in this Case, the Current of the Water point out the Direction of the Current of electrical Æther?

61. When the Machine, &c. are placed upon Originally-electrics, if a Man, standing likewise upon an Originally-electric, touches the Gun-barrel while the Globes are in Motion, he will receive a Snap or two; after which, though the Motion of the Globe is continued, he will perceive no more Fire from the Gun-barrel. While in this Posture,

* This Experiment is more elegant, if the upper Plate, attracting the Silver, is suspended high enough for a Person standing upon an Originally-electric, conveniently to bring the other Plate under it with one Hand, and to hold a pewter Plate in the other. If the Originally-electric is sufficiently thick, the Silver will not be suspended; but if the glass Siphon in a small Vessel of Water is brought very near the pewter Plate, the Water runs into the Plate, and the Silver is immediately suspended.

Posture, if he touches the Wood-work of the Machine with one Hand, and applies a Finger of his other near the Gun-barrel, at that Instant he receives the electrical Strokes. These continue as long as he touches the Machine, but cease upon his removing his Hand therefrom. Here we see a Circulation of Part of this Man's electrical Fire, which operates in the following manner. First; The Man, by applying one of his Hands to the Machine, becomes a Part thereof; and, by the Motion of the Globes, Part of the electrical Fire, inherent in his Body, is driven upon the Gun-barrel; but it is instantaneously restored to him again, upon his touching the Gun-barrel with his other Hand. Thus he continues communicating the Fire with one Hand, and having it restored to him with the other, as long as he pleases. If, instead of touching the Machine or Gun-barrel, he holds his Finger near either or both of them, you see the Fire go out, and return back, as in a former Experiment.

62. It may be perhaps imagin'd, if one Man touches the Machine; himself and the Machine both being placed upon the Wax, and if another, standing upon the Floor, constantly, or by turns, touches the Gun-barrel, that by these means the Man upon the Originally-electrics might be divested of all his electrical Fire, by constantly continuing the Motion of the Globes, as he receives then no Supply from the Floor. But the contrary proves true; and, after a considerable time, the Strokes from the Gun-barrel are as strong as at first. But here we must observe, that the Gun-barrel suspended will not contain probably, at one time a thousandth Part of the whole Quantity of
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this Man's electrical Fire: Therefore I conceive, that, as soon as this Man has parted with any Portion of his necessary, his determined Quantity, to the Gun-barrel by the Motion of the Globes, he has it restored to him upon any un-excited Non-electric's touching the Gun-barrel, by having the usual Course of the Electricity * inverted.

63. We see, from many Experiments, that dry Wood does not conduct Electricity so well as that which is wet; and that the Man standing upon the Floor, who rubs the Globes, excites the Electricity stronger than the Cushions. This I had Reason to conceive was owing not to any other Difference, than that of his being more moist, and, of Consequence, more readily conducting the Electricity from the Floor. Therefore I order'd my Machine, and even the Cushions to be made damp, by causing wet Cloths to be placed upon several Parts thereof; and found then, that the Electricity was equally strong, as when the Globe was rubbed by the Hand.

64. It remains now, that I endeavour to lay before you a Solution why our Bodies are so shocked in the Experiments with the electrified Water; the Difficulty thereof I confess seem'd unsurmountable, until I had made the following Discoveries.

1. That the Electricity always described a Circuit between the electrified Water and the Gun-barrel.
2. That the electrical Fire came from the Floor of the Room.
3. That it would not pass from the Floor quick enough for the Person to be shock, if his Shoes were dry.

4.

* For a further Account of this Matter, see *Philos. Transact.* Vol. XLV. p. 101.

4. That the Force was increased in proportion to the Points of Contact of Non-electrics with the Glafs containing the Water.

Then the Solution of this Phænomenon became more easy, which I take the Liberty to offer.

1. I have endeavoured to prove by Experiment*, that a Quantity of Electricity is furnish'd from the nearest unexcited Non-electrics, equal to that accumulated in excited Originally-electrics and excited Non-electrics.

2. This being so, when the Phial of Water held in one Hand of a Man is highly electrified, and he touches the Gun-barrel with a Finger of his other; upon the Explosion which arises herefrom, this Man instantaneously parts with as much of the Fire from his Body, as was accumulated in the Water and Gun-barrel; and he feels the Effects in both Arms, from the Fire of his Body rushing through one Arm to the Gun-barrel, and from the other to the Phial. For the same Reason, if, in the Experiment with the electrical Machine, a Man places his right Foot upon the lower small Wire, and touches the Gun-barrel with his left Arm, the electrical Force is only in that Leg and Arm.

And the Man, who the instant then parted with, is instantaneously restored to the Floor of the Room, and that with a Violence equal to the Manner in which he lost it. To confirm this, See Fig. 54.

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* See more of this in Phil. Trans. Vol. XLV. p. 102.

4. But this Flux of electrical *Æther*, either from the Floor to the Man, or from the Man to the Water, is prevented for Reasons sufficiently obvious, if the Glass containing the Water be thick; if the Points of non-electric Contact are few; if the Man is placed upon Originally-electrics; or (which is the same thing) if the Soles of his Shoes are dry.

5. As we find that the Electricity passes at least equally quick through dense Mediums, which are Non-electrics, as through those which are more lax and spongy; may we not therefore conclude, that the Cause why we feel most Pain at the Joints of our Arms, and in the Tendons of our Heels†, arises from the Texture in the Tendons and tendinous Ligaments of those Parts?

65. From a due Consideration of the *Phænomena* before us, I take the Liberty of proposing the following Queries:

1. Whether or no the Effects we observe, in Bodies being drawn to and driven from either excited Originally-electrics, or excited Non-electrics, are to be attributed to the Flux of electrical *Æther*?

2.

† This Pain in the Heels is felt only in the Experiment with the electrical Mine; and it is not perceptible only when you touch the lower small Wire with your Foot, but likewise if you stand upon Non-electrics, which touch this Wire. It has been strongly felt by a Person standing upon a Pedestal of *Portland Stone* near ten Inches in Height, and upon one of Metal more than two Feet. I am of Opinion, that no Mass of Metal, of Dimensions however great, would in the least prevent the Progress of the electrical Power from the Water in the Phials to the Body of the Man.

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2. Whether or no, that, which, from its being first discover'd in Amber, we call Electricity, electrical Æther, electrical Power, &c. is any other than elementary Fire?

3. Whether or no this Fire does not appear in different Forms, according to its different Modifications? Does it not, when diffused under a large Surface, appear to affect us as Air? When brought towards a Point, does it not become visible, as lam-bent Flame? When nearer still, does it not explode, and become the Object also of our Feeling as well as of our Hearing? Altho' it does not affect our Skin with the Sensation of Heat; does it not, by its lighting up inflammable Substances, shew itself to be truly Fire?

4. Whether or no this Fire is not connected intimately with all Bodies at all times, though least of all, probably, with pure dry Air? Have we not found and separated it from Water, Flame, even that intense one of Oil of Turpentine, Smoke, red-hot Iron, and from a Mixture thirty Degrees colder than the freezing Point?

5. Have we not proved its Subtility, from its passing through all known Bodies?

6. May we not infer its Elasticity likewise from its Explosions, from its increasing the Motion of Fluids, as well as from its Effect in the Concussion of our Bodies, when we discharge it after we have accumulated it in Water?

7. May not the electrical Machine, from its Uses, be denominated a Fire-Pump, with equal Propriety as the Instrument of *Otto Guericke* and *Mr. Boyle*, that of the Air?

8. Does not the Power we are now Masters of, of seeing the Separation of Fire from Bodies by Motion §, and of seeing it restored to them again, and even after that Motion has ceased, cause us rather to incline to the Opinions of *Homberg* (a), *Lemery* the younger (b), *s'Gravesand* (c), and *Boerhaave*,

§ The setting in of the Fire to the glass Tubes and Globes has always, in these Experiments, been visible both from the Hands and Cushions, by which they were rubbed. But as, till now, this Fire was considered as coming from the Glass, that, observed upon the Hands and Cushions, was always believed to be so much lost by running down the Instruments of Friction into the Floor. I endeavoured to prevent this Loss, by standing upon Originally-electrics; and found, to my great Surprise, that so far from increasing the electrical Power, by stopping what I conjectured was so much Loss, I could excite then no Electricity at all in the Tube and Globes. This Disappointment, which, I afterwards found, had occurred to Mess. *Bosc* and *Allamand*, was the Foundation of my discovering the Source of the Electricity, and the Manner of its Ingress to the Machine.

(a) *Homberg* du souphre principe. *Mem. de l'Acad. Royale des Sciences*, 1705. La matière de la lumière est la plus petite de toutes matières sensibles — elle passe librement au travers et par les pores de tous les corps, que nous connoissons. — Que tout l'univers est rempli de la matière de la lumière — J'ai mieux donné à notre souphre principe le nom de matière de la lumière, que celle du feu, quoique ce soit proprement la même chose.

(b) *Lemery* le fils. *Mem. de l'Acad.* 1709. p. 527. La matière de feu doit être regardée, comme un fluide d'une certaine nature, et qui a des propriétés particulières, qui le distinguent de tout autre fluide. Pag. 8. — Qu'une matière beaucoup plus subtile et plus agitée, qui remplit tous les vuides de l'univers, et ne trouve point les pores si étroits, qui ne lui laissent un libre passage, coule incessamment dans les lieux où elle est enfermée, et entretient son mouvement.

(c) *s'Gravesand Philosoph.* *Newton institutiones*, cap. 1. Ignis in corpora omnia quantumvis densa et dura penetrat. — Corporibus

Boerhaave (d), who held Fire to be an Original, a distinct Principle, formed by the Creator himself, than to those of our illustrious Countrymen, *Bacon* (e), *Boyle* (f), and *Newton* (g), who conceived it to be mechanically producible from other Bodies?

9. Must we not be very cautious, how we connect the elementary Fire, which we see issue from a Man, with the vital Flame and *Calidum innatum* of the Ancients; when we find, that as much of this Fire is producible from a dead Animal as a living one, if both are equally replete with Fluids?

10. Whether or no it is not highly probable, that, by increasing the Number and Size of the Phials of Water in a certain manner, you might not instantly

ribus sese jungit — ignem ad certam distantiam a corporibus attrahi — nulla novimus, quæ ignem non continent — non ignis æque facile corpora omnia intrat — corporibus contentus in his a corporibus circumambientibus retinetur. — Motu celerissimo ignem affici posse.

(d) *Boerhaavi Elementa Chém. de igne*, p. 187. et seq. — Ipse ignis — semper præsens existit in omni loco — imo vero in omni corpore, etiam rarissimo, vel solidissimo, æqualiter distributus hæret. — Haud ergo potui detegere, quod in rerum natura sit vel ullum spatium sine igne.

Ibid., p. 283. Huc usque conabar — tradere ea, quæ verissima addiscere potui de natura illius ignis, quem elementalem appellant philosophi. Illum scilicet, ita considerando, prout creatus ipse in rerum (natura) existet seorsum, extra reliqua omnia creata, quæcunque demum sint, corpora.

(e) Vide tractatum *De forma calidi*.

(f) *Mechanical Origin of Heat and Cold*, Sect. 2.

(g) See *Queries* at the End of his *Optics*.

instantly kill even large Animals by the electrical Strokes (*b*) ?

66. I cannot conclude these Papers, without congratulating that excellent Philosopher and learned Member of this Society the Abbé *Nollet* of *Paris*. This Gentleman, almost two Years since, in a Letter to Professor *Bose* (an Extract of which this last published with a Work (*i*) of his own) without the Knowledge of several Experiments since discover'd; at least none of his Discoveries have yet fallen into my Hands, did declare his Opinion, (*k*) that the Electricity did not only proceed from the electrified Bodies, but from all others about them to a certain Distance; (*l*) that the Electricity, as well from Bodies electrified, as from those which were not, passed more readily through dense Mediums than Air; (*m*) that the Electricity is present in all Bodies; that

(*b*) *Monf. Le Monnier* at *Paris* killed Birds by these; and with me, a Linnet and a Rat, much more than half-grown (the largest I was then able to procure) have been struck dead.

(*i*) *Recherches sur la Cause, et sur la véritable Theorie de l'Electricité*. Wittembergue, 1745.

(*k*) *Voyez Nollet dans les Recherches, &c. du M. Bose*, Pag. xlv.—La matière électrique vient non seulement du corps électrisé, mais aussi de tous ceux qui sont autour de lui, jusques à une certaine distance.

Ibid. p. xlix. — Si vous pouvez vous convaincre comme moi, que la matière qui va au corps électrique vient primitivement de tous le corps environnans, de l'air même, vous aurez bien plus de facilité à expliquer tous les autres effets.

(*l*) *Ibid.* p. xlvii. La matière électrique, tant celle qui sort du corps électrisé, que celle qui vient des environs à ce même corps, se meut plus facilement dans les corps dense que dans l'air même.

(*n*) *Ibid.* p. xlvii.

(n) that this Matter always tends to an *Æquilibrium*, and endeavours to occupy those Spaces in Bodies, which have not their necessary Quantity: All which Assertions may now be proved by Experiments.

67. You see, Gentlemen, by my asserting, that what we have hitherto called electrical *Ejfluvia*, do not proceed from the Glass, or other Electrics *per se*, I differ from *Cabeus*, *Digby*, *Gassendus*, *Brown*, *Des Cartes*, and very great Names of the last as well as the present Age. My differing from them would be Presumption indeed, were I not induced thereto, by Observations drawn from a Series of Experiments carefully conducted, to which many of you have been Witnesses, and to whom I may therefore appeal, for taking what may seem so extraordinary a Step. I have constantly had in View that excellent Maxim of Sir *Isaac Newton* laid down in his *Optics*, that, “as in Mathematics, so in Natural Philosophy, the
 “ Investigation of difficult Things by the Method of
 “ Analysis ought ever to precede the Method of Com-
 “ position. This Analysis consists in making Experi-
 “ ments and Observations, and in drawing general
 “ Conclusions from them by Induction, and admitting
 “ of no Objections against the Conclusions, but such
 “ as are taken from Experiments, or other certain
 “ Truths. For Hypotheses are not to be regarded
 “ in Experimental Philosophy. And although the
 “ arguing from Experiments and Observations by
 “ Induction

(n) *La meme.* Cette matière tend à l'équilibre, et s'empresse de remplir les espaces, qui se trouvent vuides des parties de son espece.

“ Induction be no Demonstration of general Con-
 “ clusions; yet it is the best Way of arguing which
 “ the Nature of Things admits of, and may be
 “ look’d upon as so much the stronger, by how
 “ much the Induction is more general. — By this
 “ Way of Analysis we may proceed from Com-
 “ pounds to Ingredients, and from Motions to the
 “ Forces producing them; and, in general, from
 “ Effects to their Causes, and from particular Causes
 “ to more general ones, till the Argument ends in
 “ the most general.” I am desirous, that what is
 contain’d in these Papers, you will be pleased to re-
 gard rather as the rude Outlines of a System, than
 as a System itself; which, I am in Hopes, Men of
 better Heads and more Leisure will prosecute: And
 if hereafter, from being possessed of more Observa-
 tions than we at present are Masters of, any Opi-
 nions in these Papers shall be found erroneous, I at
 all times shall be willing readily to retract them. I
 rely upon your wonted Candour, and am,

With the greatest Truth,

OÆ. 20.
 1746.

Gentlemen,

*Your most devoted
 and most humble Servant,*

W. Watfon.

F I N I S.

ERRATA.

- N^o. 459. Art. XV. Mercurius à Venere sublatus *Maii* 17, 1737.
p. 630, l. 2. this intire Article is to be expunged ; being inserted
more accurately in N^o. 450. p. 394.
N^o. 474, p. 167, l. 13. *for* Manteufet, *read* Manteuffel.
-

ERRATA in VOL. XLIV.

- N^o. 479, p. 101, l. 2. *for* foun- *read* fo un-
N^o. 481, p. 299, l. 9, *read* Grains $37\frac{4}{10}$ above 2 Oz.
N^o. 482, Sheet (Zz) the Pages are double and confus'd by a Mis-
take of the Printer ; and therefore after the Numbers of the
Pages in this Sheet I have in the *Index* set (Zz).
Ib. p. 408, l. 24, *for* Mr. Juan Antonio de Loa, *read* Don
Antonio de Ulloa.
Ib. p. 433, l. ult. *for* the other Side Chalk, *read* the other
Side of a Substance like Chalk, but much harder.
N^o. 483 in the Title *read* for the Months of *March, April, May,*
June, and *July, 1747.* *Ib.* p. 456, l. 20, *read* TAB. II.
Fig. 8.
N^o. 484, p. 630, l. 30. *read* TAB. I. Fig. 1.
Ib. p. 680. to the Mark † *add,* Sir *Isaac Newton's* Thermome-
ter was made of *Line-feed* Oil. See his Scale of Heat, *Phil.*
Transf. n. 270. p. 824.
Ib. p. 700, in the Note *dele* (p. III.)
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To the Binder.

Next after this follow the *Croonean Lectures* for
the Year 1747. and after them place the *Index* to
Vol. XLIV.

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T O T H E
Forty-fourth VOLUME
O F T H E
Philosophical Transactions,
For the Y E A R S 1746, and 1747.

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1747. by *Browne Langrish* M. D.

THE
CROONEAN LECTURES
ON
MUSCULAR MOTION,

By BROWNE LANGRISH M. D. *and* Fellow of
the ROYAL SOCIETY.

Read before the
ROYAL SOCIETY
In the Year MDCCXLVII.

Being a SUPPLEMENT to the *Philosophical
Transactions* for that Year.

There can be no greater Presumption in Favour of a Scheme,
than that it is simple, and of a Piece with the known System
of the Universe.

MEAD's *Introd. to his Essays on Poisons*, Edit. 3.

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M.DCC.XLVIII.

To the R E V E R E N D

STEPHEN HALES, D.D.

Fellow of the R O Y A L S O C I E T Y,

These THREE LECTURES

O N

MUSCULAR MOTION,

Are humbly dedicated, as a small Acknowledgment for the many and great Advantages received from his most curious, learned, and indefatigable Researches into Nature ; and for the many personal and particular Favours which he has been pleased to confer on

His Most Obedient, and

Most Humble Servant,

B R O W N E L A N G R I S H

P R E F A C E.

*I*N the Year 1733 I published an Essay on Muscular Motion, founded on the Newtonian Philosophy of Attraction and Repulsion; and though the Thoughts were hastily put together, and the Performance in general, very inaccurate, yet I must own I have not met with any Reason, since that Time, to alter my Sentiments in regard to the Cause and Manner of Contraction in the muscular Fibres.

I did not think indeed of engaging myself any further in solving such abstruse Phænomena; but being desired by my ever honoured Friend Sir Hans Sloane Baronet, to make some Experiments which might illustrate and confirm the Truth of my Hypothesis, I readily and cheerfully complied with his Commands; and afterwards I drew up the following Lectures, which were by Order of the President and Council read before the Royal Society, as the Croonean Lectures for the Year 1747.
pursuant

*pursuant to the Will of the late Lady Sadleir *.*

The Foundation of my Scheme is laid upon those Hints which Sir Isaac Newton has given us in the Queries at the End of his incomparable Book of Opticks; together with what Dr. Pemberton has said in his very learned Introduction to Cowper on the Muscles. How well it is executed is most humbly submitted to my Superiors. I do not know that I have advanced any thing inconsistent with true Philosophy; or to any of the known Laws of the animal Oeconomy. I have the Satisfaction to see, in the second Volume of the late Dr. Desaguliers's experimental Philosophy §, that he approved of what I had formerly published, beyond all other Accounts of Muscular Motion; and I must confess, that meeting with Countenance from so good a Judge gave me great Encouragement to pursue the Enquiry.

** Whose first Husband was Dr. Croone, and at his Request these Lectures were founded.*

§ Page 393.

THE
CROONEAN LECTURES
ON
MUSCULAR MOTION.

LECTURE I.
SECT. I.

*Read at a Meeting of
the Royal Society, on
March 5, 1746-7.*

THE external Superficies of every Muscle, its Origin and Insertion, the Use of its Action in regard to the Animal Oeconomy, from what *Plexus* the Nerve comes, whence the Artery which supplies it arises, and to what Vein the Blood is carried, are Disquisitions very worthy the Knowledge of every *Physician* and *Surgeon*; in order to discover the true Seats and Causes of many Complaints, and to point out the most rational Methods of Cure.

II.

But as these Researches have nothing to do with the immediate Cause of Muscular Motion (the Ex-
A plication

plication of which seems to be the chief Design of the Learned *Founder* of these Lectures) I shall wave all such Enquiries, and proceed directly to investigate by what Means a Muscle so instantly and forcibly contracts itself, or shortens its Length, at the Command of the Will; and this I shall endeavour to do by such Rules and Laws as are simple, uniform, and intirely agreeable to the known System of the Universe.

III.

In order to the easier Illustration of this wonderful and important Property in the Fibres, I shall give you, *1st*, An analytical View of the component Parts of a Muscle. *2dly*, I shall shew the true Cause of Cohesion, Tension, and Elasticity in the animal Fibres. *3dly*, I shall make it appear, that every Fibre constituting a Muscle, is, in its ultimate Division, tubular, and not a Chain of Vesicles or Bladders. And, *lastly*, I shall prove it to be more than probable, that Muscular Motion proceeds from the *attractive* Quality of the constituent Particles of every Fibre being increased and strengthened by the Addition of some *athereal Matter* flying out from the Extremities of the Nerves; and that this is instantly occasion'd by the Will, and ceases again as soon.

• IV.

By a chemical *Analysis* of a Muscle dissected from the Buttock of a lean Ox, which weigh'd exactly two Pounds *Averdupois*, I procured,

[3]

	Oz.	Dr.	Gr.
1. Lymph ——— ———	xxiiii	xiii	xv
2. Volatile Salt ——— ———	i	iiii	xxv
3. Oil ——— ———	ii	vi	xx
4. <i>Caput mortuum</i> ——— ———	ii	ix	v
5. Lost in Distillation, which I } presume was mostly mere Air }	——	xii	xvi
	32	00	00

V.

There being no *Averdupois* Weights in the Shops, less than Quarters of Ounces, I order'd some to be made of a Drachm, and others of two Drachms. The Drachm weigh'd 27 Grains; so that, by casting up the Grains into Drachms, and the Drachms into Ounces, we have the exact Weight of each of the above constituent Principles according to the Weight they were first of all weigh'd with.

VI.

It may be proper also to observe, that the *Apparatus* I made use of in this Process, was the same which I communicated a Description of to the *Society* some time ago, and which is since published in the *Philosophical Transactions* N°. 475; except that the Retort I now used was made of Copper, in order that I might remove it from the the Sand-Heat into the actual Fire, without unluting any Part of

[4.]

the *Apparatus*, when no more Matter would arise by means of the Sand-Heat.

VII.

By this Method I could increase the Fire till the Bottom of the Retort was red-hot, without any Danger of breaking my Recipients; a Contrivance which may be useful in many chemical Processes.

VIII.

From the above-mention'd Experiment we have evident Proof of the Proportions and Qualities of the several Principles, or constitutive Parts of the muscular Fibres; and let no one be surpris'd that the watery or phlegmatic Principle abounds so much as to be nearly $\frac{7}{8}$ Parts of the whole Mass, since we know that dried Bones, and many other Things as unpromising, afford half their Weight of Water.

IX.

That the Particles of Water are endued with a strongly attracting Power, and are highly serviceable as a Band of Union in the Formation and Growth of every thing, animate and inanimate, is not only manifest from the great Quantity employed in the Growth of Animals and Vegetables, but also in our own manual Operations, such as making several Sorts of Glue, Pastes, Bricks, and such like, where the watery Particles prove a very durable and powerful *Copula*, and are not to be all of them separated again,

again, even by a very intense Fire: Water is to be regenerated from Bricks and Tiles after they have been burnt in the Kiln.

X.

The driest Wood, Part of a Mahogany-Table, which had stood by the Fire many Years, being rasped and put over the Fire in a Copper Retort, afforded a considerable Quantity of Water.

XI.

In short, take away the Water from the most solid animal and vegetable Bodies, and they immediately become mere Dust.

XII.

I don't apprehend that this considerable Quantity of Water, which is to be procured from such solid Substances as Bricks, Wood, or even from the muscular Fibres, remains in distinct Drops or Spherules, whilst it is a Part of such solid Substances; but I conceive that the *Minima*, or primary solid Particles of Water may be attracted by, and actually joined with, the earthy, saline, and other component Particles, so as to compose the several Degrees of Hardness, according to their respective Proportions and Qualities; and when these several constituent Principles are disunited again, by the Power of Fire, or by the Length of Time, they rise up into the Air, or into
the

the Recipient, according to their Divisibility and Levity; first Water, next Salt, then Oil.

XIII.

Nature seems to delight in Transmutations. Many Kinds of Fluids are easily converted into dense Bodies. We all know how soon Water is capable of being turn'd into a very solid friable Stone, by the Power of Cold. Mercury also is easily turn'd into a hard brittle Metal; and both these return to their former State of Fluidity by means of Heat. And a Solution of Copper in Spirit of Nitre being pour'd on Oil of Tartar, both Liquids instantly become Verdigrease in a dry Powder.

XIV.

From what has been said we may observe, that Water, or the watery Particles not only make up much the greatest Part of the muscular Fibres, but, by mutually attracting, and being attracted by the other component Particles, they greatly contribute towards their Cohesion and Elasticity; for a fluid Particle will be fixed, and become a Part of a Solid, as soon as there is an attractive Force sufficient to effect its Cohesion with the other solid Parts, though it returns to its former State of Fluidity upon the Analysis of the compounded Body.

XV.

I would not by this be understood as if I design'd to exclude the other Principles from their Share
which

which they give to the true Degree of Firmness and Elasticity in the Fibres; the saline, sulphureous, and earthy Parts are all endued with a strongly attracting Power; and when brought into Contact with each other, or with the watry and aerial Particles, they give Firmness and Solidity to the Composition.

XVI.

Water seems to be the *Gluten* by which the other Principles are wrought up. Too much Water in the Composition renders the Fibres soft and lax; as in Children, and anasarious People. Too little Moisture occasions a stiff, rigid Fibre; as in old Age. There is a certain Degree of Texture and Cohesion necessary towards muscular Motion in its greatest Strength.

XVII.

I have shewn in a former Treatise*, that Air is very instrumental in fixing and uniting the other Principles which constitute an animal Fibre; for in the most solid Parts of the Body, where the Cohesion is strongest, we find great Plenty of Air. That the Air-Particles are capable of being united, and fix'd to solid Bodies, and by that means may be esteemed a Part of their Composition, we have many evident Proofs in Dr. *Hales's Analysis of the Air*; and that those Particles do in their fixed State
strongly

* Modern Theory of Physic, p. 56.

strongly attract the other component Particles, is evident, it being well known, that the most strongly repelling and elastic Particles, when in a separate State, are the most strongly attracting, when fixed to other Bodies.

XVIII.

Now, according to Dr. *Hales*, since a much greater Proportion of Air is found in the solid than in the fluid Parts of Bodies; may we not with good Reason conclude, that it is very instrumental, as a Band of Union in those Bodies; those Particles (as Sir *Isaac Newton* observes*) receding from one another with the greatest repulsive Force, and being most difficultly brought together, which upon Contact cohere most strongly? And if the Attraction or Cohesion of an unelastic Air-Particle be proportionable to its repulsive Force in an elastic State, then since its elastic Force is found to be vastly great, so must that of its Cohesion also. Let us add to this, that the Air generated from the fleshy Fibres, in the Experiment above-mention'd, was not separated without great Violence; for it did not rise in any Quantity, till the Clouds did, which contain'd and brought over the Salt and Oil: Whence it is evident that the æreal Particles are firmly fix'd, and consequently are very instrumental in the Union of the other constituent Principles.

XIX.

XIX.

Sir *Isaac Newton* * thinks, that not only Water and Air are convertible into dense Bodies, but that even Light may become a Part of gross Bodies, and that they may receive much of their Activity from the Particles of Light which enter their Composition. It is the Opinion also of Monf. *Homborg*, that Light or Fire is a Part of the Composition of all Things; though in the Analysis of Bodies it is always lost, escaping the Skill of the Artist, and passing through the closest Vessels.

XX.

We all know that solar Fire, or Light, adds Weight to Lead, Tin, or Regulus of Antimony, when exposed to the Fire of a burning Glass, though they otherwise lose much in Smoak and Steam. But to proceed :

XXI.

These then being then the component Parts of the muscular Fibres, our next Task is to shew the Cause of their Tension and Elasticity.

XXII.

That all the muscular Fibres of the Body are in a State of Tension, during Health, is manifest from

every

* Opt. Qu. 30.

every Incision made across them, when the two Segments of the Muscle so divided, retire, one to its Insertion, and the other to its Origination; that is, every Fibre is always stretched out beyond its natural State of Rest or Quiescence, so that both Ends of it retract a considerable Distance after being cut asunder. Now there are two Things which seem to be principally concerned in this Affair; *viz.* the Impulse and Pressure of the circulating Fluids, always distracting the Fibres, and a constant *Nisus* or Endeavour in the constituent Particles of the Fibres to run closer together, when so distended, by means of their mutual Attraction towards each other.

XXIII.

The Equilibration which is ever preserved between the antagonist Muscles, in a healthy State, unless when the Will directs it otherwise, arises from this *Vis Restitutionis*; which being stronger or weaker according to the Degrees of Tension, and the Degrees of Tension depending upon the Velocity and Quantity of Fluids circulating through every Fibre; it follows, that as long as the Fluids have the same free Access to every voluntary Muscle, so long will the *Equilibrium* be maintained.

XXIV.

In an Essay which I publish'd on this Subject of muscular Motion, in the Year 1733, I endeavour'd to prove that every the least Corpuscle of Matter is endued with an attractive *Virtue* on one of its Sides,
and

and a repulsive Power on the other, something similar to the Loadstone; and this I was first of all induced to believe, from what Sir *Isaac Newton* observes in his *Opticks*, Qu. 31.

XXV.

When any saline Liquor (*says he*) is evaporated to a Cuticle, and let cool, the Salt concretes in regular Figures; which argues, that the Particles of the Salt, before they concentered, floated in the Liquor, at equal Distances, in Rank and File; and by Consequence, that they acted upon one another by some Power, which at equal Distances is equal, at unequal Distances is unequal: For, by such a Power, they will range themselves uniformly, and without it they will float irregularly, and come together as irregularly. And since the Particles of *Iceland* Chrystal act all the same Way upon the Rays of Light, for causing the unusual Refraction, may it not be supposed, that in the Formation of this Chrystal, the Particles not only ranged themselves in Rank and File for concreting in regular Figures, but also, by some kind of polar Virtue, turned their homogeneal Sides the same Way?

XXVI.

And again, we are taught by the same *Great Man*, that Fire is the most simple of all known Bodies, and consequently the most immutable; that each Ray of Fire or Light has Sides differently affected, and which have different Properties; and that *Iceland*

Chryſtal is ſound to attract a Corpufcle of Fire, if one of its Sides be turned towards the Chryſtal, and repel it, if the other be; for one and the ſame Ray is here refracted ſometimes after the uſual, and ſometimes after the unuſual manner, according to the Poſition which its Sides have to the Chryſtal; and ſince the Chryſtal, by this Diſpoſition or Virtue, does not act upon the Rays, unleſs when one of their Sides of unuſual Refraction looks towards that Coaſt, this argues a Virtue or Diſpoſition in thoſe Sides of the Rays, which answers to, and ſympathizes with, that Virtue or Diſpoſition of the Chryſtal, as the Poles of two Magnets answer to one another.

XXVII.

We are fully perſuaded, that, in the Chryſtallization of Salts, they could not ſo regularly and conſtantly preſerve their peculiar Shapes, Forms, and Figures, if every Particle of them had not its determinate Poles: For granting that the component Particles of each kind of Salt have a peculiar Shape and Size, different from any other kind of Salt, yet if they had a Power of uniting with each other indifferently, at their Tops, Sides, and Bottoms, one would think they could not always coaleſce into Chryſtals of the ſame regular Figure: But if the conſtituent Particles of every kind of Salt have their determinate Poles, then they cannot poſſibly unite with each other, but when their Poles ſquare with one another, and conſequently they will always fly together, and be joined at ſuch Points, only where their correſponding Poles are, which muſt of courſe conſtantly produce the ſame regular Form and Figure
in

in every Aggregate of such particular saline Particles.

XXVIII.

Hence it is, that *Salt Armoniac* so elegantly imitates the Branches of a Tree; *Salt of Hartshorn* a Quiver of Arrows; *Salt of Tin* shoots into Lines like little Needles, which spread themselves every Way from a Point, as from a Centre, so as to represent a Star, &c. Now can it be imagined that these, or any other kind of Salts, would immutably and perpetually coalesce into Chrystals of the same regular Figure and Shape from any other Principle?

XXIX.

Since therefore we have so much Reason to believe that Salts of all kinds, and even the Rays of Light are endued with a polar Virtue, that is, every Corpuscle attracts on one of its Sides, and repels on the other; and since it is a well known Axiom, that *Nature is ever frugal in Principles*, I think it not at all unphilosophical, or contrary to any of the known Laws of Nature, to believe that every Particle of Matter in the World is endued with an attractive and repulsive Property.

XXX.

Thus then, if the constituent Corpuscles of the muscular Fibres are formed together according to this Law, if they are all united at particular Points
cor-

corresponding to their attractive Virtue, it follows, that wherever a muscular Fibre is stretched out to the least Degree of Tension, some of its Particles will touch each other in fewer Points; whilst others may possibly be disunited and removed from each other, though perhaps to inconceivable small Distances: Hence there will be a constant *Nisus* in the separated Particles to get together again; and this *Vis Restitutivæ* will be stronger or weaker, according to the Number of Corpuscles so disjoined, and their attractive Virtue.

XXXI.

If the Power of the circulating Fluids (and I think it cannot be denied) be sufficient, from the first Beginning of the Circulation of the *Sarcs*, and so on as long as Life continues, to distend the Fibres beyond the Size they would otherwise be of, by reason of their corpuscular Attraction; this distracting Power must always be the Occasion of some Degree of Tension in them: And if, upon the Removal of this Tension, the component Particles have a Property of running closer together, and contracting the Fibres in their Length, by the means above-mention'd, this must be the true Cause of Elasticity in the Fibres.

XXXII.

Hence therefore it follows, that since the Fibres are always in a State of Distraction, by the Quantity and *Momentum* of the circulating Fluids, and as they are ever endeavouring to shorten themselves, by means of their corpuscular Attraction, their Elasticity must

must depend upon Tension; for the Fibres could have no Power to retract, or abbreviate their Length unless they were extended beforehand by some certain Force.

XXXIII.

It is not a sufficient Objection against this Scheme to say, that if we depend upon what is visible, we shall never see the dry solid Fibres, or Particles of any solid Body, once divided or drawn out of Contact, coalesce or unite again, or recover the close Contacts they had before; without some fluid Medium superadded.

XXXIV.

'Tis true, when a visible Crack or Flaw happens in any dry, hard, solid Body, such as a Steel-Spring, or a dry, rigid, wooden Bow, the Rupture will always continue, by reason the sever'd Particles cannot be brought again into the Sphere of each other's Attraction without the Intervention of some Medium; but then it does not follow from hence, that such a Spring or Bow cannot be bent at all without breaking; or that the constituent Particles, which must necessarily be displaced by such a distending Power, do not fly together again by their attractive Virtue, when removed only to such minute Distances.

XXXV.

The *Minima*, or primary Atoms of all Bodies are non-elastic, as being perfectly hard, solid, and inseparable; and therefore Elasticity must proceed from the Aggregate, or Composition of such Atoms, which,
by

by being capable of changing their Situations, according to the impressed Force, and being endued with a powerful attracting Virtue, they instantly resume their former Positions, when left to themselves to obey those Laws which the *Great Creator* hath impress'd upon them. As for Instance: Whilst a common Steel Spring, or any such elastic Body, is not extended or bent, we presume every individual Particle of it to be at Rest; that is, they are all situated, in regard to each other, according to their Poles, and embrace one another by their common Principle of Attraction; but no sooner is such a Spring bent, by some impressed Force, but many of its Particles on the convex Side, must of course touch in fewer Points, or perhaps be disjoin'd from each other, though to the most minute Distances that can possibly be; whilst other Particles, on the concave Side of the Spring, must necessarily slip upon, or be crowded over one another. Hence it will follow, that if those Particles which are separated from each other, or touch one another in fewer Points than usual, are yet so near each other as to be within their Sphere of Attraction, and, not at all, or very little alter'd in regard to their Poles, they will consequently attract each other very strongly, and fly together again, as soon as the impressed Force is removed; whereas it is no unreasonable Conjecture to suppose, that those Particles on the concave Side of the Spring, which are compressed, and as it were rumpled over one another, may be so much alter'd from their former Positions, that their Poles do not now answer to each other; and if not, they will repel one another, according to their respective

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Power

Powers, till they have attain'd their former Situations, or, in other Words, till the Spring has recover'd its former Shape.

XXXVI.

The same Principles of Attraction and Repulsion are the Cause of Restitution or Elasticity in all other kind of Bodies. When a muscular Fibre is stretched out longer than usual, it is most certain that some of its component Particles must slip upon or by one another, or else be removed at exceeding small Distances from each other; so that if the impressed Force be too violent, if the Tension be carried so far as to disjoin a great Number of the component Particles beyond their Sphere of Attraction, the Fibre will continue to grow weaker and weaker till it breaks: But it is as evident, on the other Side, that when a stretched-out Fibre does not break, but retracts itself into its former Shape and Dimensions, upon the Removal of the extending Power, the Particles which were displaced return again to their proper Positions, merely by the means of their attractive Virtue.

XXXVII.

Now all this being so agreeable to those *Laws of Nature* which that divine Man Sir *Isaac Newton* has discover'd to us, I think we have good Reason to conclude it to be the true Cause of Elasticity in the animal Fibres.

XXXVIII.

The Elasticity in the Air indeed, or in Water agitated by Fire, or in all the Exhalations proceeding from the Earth, arises from the Principle of Repulsion only; for the Particles of Vapours, Exhalations, and Air, stand at a Distance from one another, and endeavour to recede as far from one another as the Pressure of the-incumbent Atmosphere will admit them. No Power yet known is able to compress the Air-Particles within the Sphere of their Attraction towards each other, so as to destroy their elastic Property; and yet single primary Particles of Air are continually attracted by other Bodies, and consolidated with them, till by the Action of Fire, or Fermentation, they are separated again, and restored to their repulsive State.

XXXIX.

Hence we may observe, that Elasticity, in different kinds of Things, or in Matter differently modified, may arise from two several Causes, *viz.* *Attraction* and *Repulsion*; and perhaps, in many Instances, from the Influence of both at one and the same time.

XL.

Whenever any kind of Matter is actuated by Fire, by Fermentation, or dissolved by any Menstruum, so as to throw off its Particles in subtile Vapour, there
will

will be a constant Endeavour in those Particles to recede further from each other; so that the more they are confined, or compressed, the greater will be their elastic Power: Whereas in solid Bodies, this Property of Elasticity proceeds chiefly from Attraction, or a *Nisus* in the component Particles to fly back, or run into close Contacts again, whenever they happen to be stretched out, or bent, so as to touch each other in fewer Points.

XLI.

From what has been said we may deduce the following *Corollary, viz.* That whenever *Elasticity* proceeds from the Principle of *Repulsion*, as it does in Air, Vapours, &c. some *Compression* is necessary, in order to force the elastic Matter into a narrower Compass than it would otherwise possess; but when it arises from *Attraction*, as in the muscular Fibres, and all solid Bodies, some *distractile Force* is requisite to disjoin the component Particles from their usual Contacts, before it can exert its Power; and perhaps, for want of attending to this Difference, so many various Opinions may have arisen concerning the Cause of Elasticity.

XLII.

In my next LECTURE I shall consider the Shape of the muscular Fibres, and the Cause of muscular Action.

LECTURE II.

XLIII.

Read March 26. 1747. **I**N my former LECTURE I endeavour'd to explore the several constituent Principles of the muscular Fibres, and to shew the true Cause of their Cohesion; Tension, and Elasticity. In this, I hope, I shall make it appear, *1st*, That every Fibre constituting a Muscle is *tubular*, and of a *cylindrical* Shape, or very nearly such; and not a String, or Chain of Bladders, according to an *Hypothesis* which has been too long and too generally received. *2^{dly}*, That the corpuscular Attraction between the component Particles of the Fibres is so far increased and strengthen'd by the Influence of the nervous *Aether*, which is always at the Command of the Will, as to purse up and shorten every Fibre in its Length, whereby an Intumescence arises in the Belly of the Muscle, though it is contracted in its other Dimensions, so as, in the Whole, to possess less Room.

XLIV.

Being favoured with the Use of a most excellent Microscope, I made the following Experiments.

1st,

XLV.

1st, I divided some Fibres as minutely as I possibly could, from the Heart of an Ox, from a Part of the Diaphragm, from the intercostal Muscles, and from a Rump of Beef; all which were boiled to such a Degree of Tenderness, that we separated some Fibres with the Point of a Needle, which were not visible till placed under the Microscope, and even then they did not appear bigger than Hairs, though others, which looked like Hairs to the naked Eye, were magnified to the Size of Wheat-Straws. All these seem'd to be Fascicles of continued Tubes, as far as we could view them, without any Partitions or Cells.

XLVI.

2^{dly}, Upon rending a Muscle, which was taken from a Knuckle of Veal, and boil'd for four or five Hours, several of the transverse, as well as longitudinal Fibres appear'd very distinctly; which being placed under the Microscope, and having a strong focal Light cast upon them by means of a *Florence* Flask fill'd with Water, they seem'd to be shrunk up, either by being boil'd so long, or by being exposed to the Air, so that their Surfaces seem'd to be unequal and corrugated; which is what *Mr. Leewenhook* * says deceived him at first, so as to make him

* *Anatom. & Contempl.* p. 43.

him think these Corrugations were so many Vesicles or Cells; but he soon discover'd his Mistake. In some of the Fibres I could plainly discern a dark List running in the Centre, from one End to the other; but what it was, I could not discover.

XLVII.

3dly. Having observed the muscular Fibres in the Leg of a Sea-Crab to divide very easily and distinctly from one End of the Muscle to the other; we placed a great many of them under the Microscope, but could not discern any thing like Partitions or Cells.

XLVIII.

In short, Mr. *Leeuwenhoek* * assures us, that the minutest Fibres that are visible to the naked Eye appear through a good Microscope to be invested with a Membrane, which includes within it not one simple Body, but a Bundle of still finer Fibres, the last, or smallest Order of which he thinks to be simple hollow Tubes.

XLIX.

This perhaps is as good Authority as we can have from the Assistance of Microscopes; but if we may be allowed to deduce our Arguments from the Analogy which the muscular Fibres bear to some other
Parts

* *Phil. Trans.* N^o. 367.

Parts of the Body, whose Shapes we are well acquainted with, the Reasonableness of this Opinion may appear yet stronger.

All Anatomists agree, that the muscular Fibres have their Rise from the Extremities of the Nerves and Blood-Vessels; every Fibre being supplied by a Branch of a Nerve, and an Artery, and having also a Vein arising from it.

LI.

That the nervous *Capillamenta* are Cylinders is not denied by any one that I know of; and though the Arteries have been for a great while thought to be conical, yet the ingenious Dr. *John Stephenson* * Fellow of the *Royal College of Physicians* at *Edinburgh* hath evidently demonstrated the whole arterial System to be Cylinders, frequently divided and subdivided, still terminating in Numbers of small Cylinders, the Aggregate of which is always of greater Capacity than the Trunk or larger Cylinder before the Ramification.

LII.

May we not therefore very reasonably believe, from the Simplicity and Uniformity in all the Operations of Nature, that the muscular Fibres partake

of

* Medical Essays, Vol. V.

of the same Figure with those from whence they have their Rise; especially when such a Shape (as will appear in the Sequel) is more proper for all the Functions of a Muscle than any other whatsoever?

LIII.

I don't mean by this, that every Fibre of every Muscle is a perfect and regular Cylinder from one End to the other; many of them may be thicker in their Coats, and larger in their Bores about the Middle than towards each End, similar to the Shape of the Muscle; but what I think the most reasonable Opinion is, that the smallest *Fibrillæ* are hollow Tubes not divided into an infinite Number of Cells or Vesicles.

LIV.

The longitudinal, red, fleshy Fibres seem indeed to be contorted and bound about in many Places, with white, spiral, and transverse Ramifications of the Nerves; but I can see no Reason to believe that these nervous Filaments divide the longitudinal fleshy Fibres into several Apartments or Cells; I rather think that they only dip into the Cavities of the Fibres, in order to convey into them the *æthereal Medium*, which is contained in the Nerves.

LV.

Before the Laws of Nature, and the Animal Oeconomy were so well known as they are now, I don't wonder that the vesicular Scheme was thought a reasonable one, till it came to be examined by strict Rules and Experiments. The common Experiment of raising Weights by blowing up Bladders might seem, at first Sight, a very feasible Way of explaining muscular Motion; and without Doubt this first of all gave Birth to the vesicular *Hypothesis*.

LVI.

But the Fallacy of this Experiment was not discover'd for want of attending to the Difference between Bladders which have been already blown up, and dried, and such as are recent and supple.

LVII.

If a String of dry Bladders, which have been once distended as far as they could bear without bursting, and are now again squeezed close, and stretched out only in their Length, by means of a Weight hung at their Bottom; I say, if such a String of Bladders be blown up, it will undoubtedly distend their transverse Diameters so as to raise up the Weight: But in all tender yielding Vesicles, such as the muscular Fibres most certainly are, in their last, or smallest Order, it is well known, that if they were to be inflated with Air, or any such-like Matter, it would

D distend

them in every Direction alike; they would grow longer as well as wider. Hence it follows, that if the abovemention'd Experiment was to be made with Bladders just as they are taken out of animal Bodies, it would not answer the Purpose, as is evident from blowing up those of *Calves*, *Hogs*, &c.

LVIII.

The muscular Fibres, it is true, are always in a State of Tension, but then this Tension is very far from being to their utmost Stretch; so that, were they to be inflated in the manner above-mention'd, every Muscle would necessarily increase in Length as well as Breadth.

LIX.

Another insuperable Difficulty belonging to the vesicular *Hypothesis*, is how to blow up a Bladder open at both Ends; which every Vesicle is supposed to be, by having a free Communication with the Blood-vessels.

LX.

Having therefore so much Reason to conclude, that the muscular Fibres, in their ultimate Divisions, are not cellular, but tubular, let us proceed to shew the Manner and Cause of their Contraction.

LXI.

A Muscle in its Motion very evidently grows less in Bulk.

This Proposition is clearly demonstrated by that famous Experiment communicated to the *Royal Society*, by Dr. *Goddard*† in the Year 1669, where putting a Man's Arm into a glass Cylinder full of Water, the Water always sunk when the Muscles of the Arm were contracted, and rise again to the first Standard when they were relaxed. This we think may be look'd upon as an *Experimentum Crucis*; whereas, if every Fibre was a Chain of Bladders, whose Contraction in Length arose from their Inflation in Breadth, all the World knows there would be a sensible Swell of the whole Arm upon muscular Action.

LXII.

There are still other Difficulties attending the vesicular *Hypothesis*. If the animal Spirits are supposed to inflate the Cavities of the muscular Fibres merely by a propulsive Force, like unto the Steam of boiling Water working in the Engine to raise Water by Fire, it ought to be proved from whence so strong an Impulse should arise; and also how the Nerves, which are the Conduits thro' which this
flatulent

† *Vide Register of the Royal Society*, Vol. IV. p. 95.

flatulent Matter must be convey'd, should lie so loose and unelastic; it being evident from all Experience, that if such an elastic flatulent Vapour was to fly thro' the whole Length of the Nerves, with an Energy sufficient to give a Man a Power of lifting up great Weights, the Nerves must be stretched out in Proportion, and consequently would be very tense and elastic.

LXIII.

Those who suppose the Inflation of the Muscles to arise from a fermentative Motion in the Fluids, ought to prove, by a proper Number of Experiments, that there are Juices existing in the Body capable of such sudden and violent Rarefactions or Explosions, upon mixing with each other; and if this possibly could be done, the Diminution of the Bulk of the Muscles in Action, would overturn all their Scheme.

. LXIV.

Hence it is evident that the *vesicular Hypothesis* ought to be intirely rejected, as being repugnant to the *Laws of Matter*, and to the *Phænomena* of the Muscles.

. LXV.

By undoubted Experiments we are convinced, ~~that the~~ Intumescence of a Muscle, when it acts, arises merely from a Change made in its Figure; that is, as it shortens in Length the Belly grows thicker, and yet the Bulk in general is diminished.

Let us therefore inquire after the Agents which are capable of producing such surprising *Phænomena*, and at the same time shall be consistent with every other Operation in the Animal Oeconomy.

LXVI.

From what has been said it appears, that Contraction, or muscular Action, does not depend upon any Fluid dilating or distending the Fibres; but, on the contrary, they *shrink up* and grow less. The instantaneous Alternations from Constriction to Dilation, and *vice versa*, manifestly discover that muscular Motion cannot be caused by such Juices as the *Blood*, *Lymph*, and such-like; but it must be from some more subtile *æthereal* Matter, which may be mix'd with the Blood in general, and secreted from it by the Glands of the Brain.

LXVII.

Let us but carefully consider the exquisite *Apparatus of the Brain*, the Quantity of Blood it receives, the infinite Number of its excretory Ducts, and the great Divisibility and Subtily of Matter, and we shall find great Reason to conclude that there is a most subtil, *æthereal*, volatile Fluid, of great Force and Elasticity, perpetually secreted from the Blood, by the Glands of the Brain, and, continually flying into the Nerves, for the Use of muscular Motion, and many other great Purposes of the Animal Oeconomy.

LVIII.

LXVIII.

The delicate Texture of the Nerves, as well as that of the Brain, implies that the Fluid they convey to the Muscles must be exquisitely fine. Indeed when a Nerve is wounded, there flows from it a sweet, soft, clammy, balsamic Juice, which no doubt is carried, at all other times, by the evanescent Nerves to their ultimate Divisions, in order to nourish and preserve the most minute *Fibrille*, and all their Expansions; and this may properly be called the *Succus nutritius* of the Nerves. But I cannot conceive that this visible Juice has any thing to do with the immediate Cause of voluntary Motion; for so viscous a Matter could never admit of such sudden Vicissitudes, as are in muscular Action, if it was capable of performing it in other Respects.

LXIX.

There are Abundance of Considerations which evince the Existence of some subtil Spirit in the Nerves, much finer than to be the Object of our Senses. We have no Proof, either from Experiment or Reason, of any other Instrumental or physical Cause of Sense or Motion, but this *animal Ether* which is elaborated from the Blood.

LXX.

The learned Dr. *Mead* * thinks no Regard ought to be had to the immechanical Notions of those Authors, who imagine that there is no such thing as a nervous Fluid in an animal Body ; and that muscular Motion and Sensation are performed only by the Vibrations of the Fibres of the Nerves, without the Intervention of any spirituous Fluid.

LXXI.

The surprising Discoveries which have been made of late Years, by a Variety of Experiments upon Electricity, do in some measure give us an Idea of the great Subtily and Velocity of the nervous Fluid. I have been informed by the ingenious Mr. *Watson*, a worthy Member of this *Society*, that the Swiftness of the electrical *Effluvia* is prodigious ; that one Stroke of his Hand down the Tube, when well electrified, was felt as soon as his Hand could be at the Bottom of the Tube, through five Men standing upon electrical Cakes, and communicating with each other by a Cane, Sword, or any other Non-electric.

LXXII.

Hence it follows, that if a Tube could be always excited, and was always to be applied to the End of

a

a proper Cord or String; the electric Matter, which is excited by Friction between the Hand and Tube, would ever be ready to exert its attractive Influence on Leaf-Gold, and such like Things, when placed within a due Distance of the End of the String; and perhaps this may be very similar to the Motion and Action of the nervous *Æther*.

LXIII.

Thus much being premised, and it being taken for granted, that we have an *æthereal Medium* in the Brain, Spinal Marrow, and all the *Capillamenta* of the Nerves, ever ready to be convey'd into the muscular Fibres, by the Power of the Will, and which *Medium* consisting of the most refined Matter in Nature; it follows, that the Motion of this nervous *Æther* may be as quick as Lightning, and alſo its attractive Power must be exceeding strong, by virtue of its vast Degree of Subtilty; as is evident from what Sir *Isaac Newton* * has calculated concerning the Rays of Light.

LXXIV.

From these Observations therefore, and from what has been said above concerning the Cohesion and Elasticity of the animal Fibres; I think we have great Reason to conclude, that muscular Motion does proceed from the Influence which the nervous *Æther* has

* Opt. Qu. 23, 24.

upon the component Particles of the muscular Fibres themselves, by instantly increasing their attractive Virtue towards each, so as to make them run closer together, or, as it were, up into Heaps, as long as such an additional attractive *Medium* is in the Fibres.

LXXV.

If we look back and consider the Nature of the *Vis Restitutivis*, or Elasticity in the muscular Fibres, we shall find, that the Cause of that Property only wants to be increased, in order to overcome the distractile Force of the circulating Fluids, and the Resistance of the antagonist Muscles: Whence it follows, that if such a Power was to be increased in one Set of Muscles, and not in their Antagonists, those Muscles, whose elastic or retracting Power was increased, would be abbreviated in their Length, whilst the others would be extended and lengthen'd.

LXXVI.

When any Muscle is freed from the Power of its Antagonist, by a Wound, &c. it immediately contracts, and is not to be extended again by the Power of the Will. Whence it has been said, that Contraction is the proper State of the Muscles, and to which they always tend: But if we narrowly inspect into this Affair, we shall find, that, when a Muscle is contracted in this manner, it is no further so than the elastic restitutive Property in the Fibres is concerned. We do not find that such a Muscle is indurated, or its Belly swoln like unto what it is in

voluntary Action; for here being no Influx of the nervous *Aether* to increase the corpuscular Attraction, the Muscle is shortened only by the inherent mutual Attraction between the constituent Particles of its Fibres, without any Matter being superadded. This kind of Contraction therefore is evidently the State to which the elastic Fibres, tend by a continual *Conatus* in the component Particles to accede towards each other without the Assistance of the nervous *Aether*; so that this natural *Vis motrix* in the musculous Fibres is no more than what we mean by their Elasticity, or restitutive Property: It seems however to be demonstrated from hence, that muscular Action, and Elasticity in the Fibres, proceed from the same Cause in different Degrees; *viz.* from corpuscular Attraction.

LXXVII.

Let us now endeavour to corroborate these Arguments by some suitable Observations.

LXXVIII.

1st. From what has been said we may conceive more readily, than we know how to express, that the Will has a Power to direct the *æthereal Medium* contain'd in the Nerves, to any of the voluntary Muscles, with such a Degree of Celerity, as it pleases; and to stop the Influx as suddenly.

LXXIX.

2nd. It is evident that the Coats, or carnos Substance of every Fibre, must necessarily increase in its Thickness,

Thickness, when it abates in its Length; and what Power can produce this Effect, but such a one as increases the mutual Attraction between the constituent Particles?

LXXX.

3dly. Hence appears the Reason, why the Belly of a Muscle swells during its Contraction, notwithstanding its Dimension in general is diminish'd; for as the component **Particles of each Fibre** are more loosely join'd together about the Middle than towards its Extremities, which are generally tendinous, it is natural to suppose that the chief Action is between them; that is, when a Fibre grows shorter, such of its Particles which are most at Liberty run nearer together, and as the Motion of all Bodies is ever in proportion to the Impulse they receive, and the Resistance they meet with, so when the constituent Particles of the muscular Fibres are drawn into a shorter Compass, by the means above-mention'd, the Middle of the Fibres must swell either inwardly or outwardly, or both, according to the Resistances they meet with.

LXXXI.

And *lastly*, Since the Coats of the muscular Fibres do most certainly grow thicker as they contract in their Length, and yet the external Surface of the Muscle in general is diminished; it manifestly follows that their Cavities must grow less, and their

contain'd Fluids must be pressed out, in proportion to the Contraction of the Muscle.

LXXXII.

This appears upon Blood-letting, when squeezing any thing hard in the Hand will make the Blood fly out with a greater Velocity, and thereby form a larger *Parabola*.

LXXXIII.

This also accounts clearly for the Induration and Paleness of a Muscle during its Action.

LXXXIV.

And again, it follows hence, that in the Action of the Muscles there is an alternate *Diastole* and *Systole* perfectly analogous to the Action of the Heart, which greatly contributes towards pushing on the Blood in the Veins.

LXXXV.

The Muscles being contracted merely by the Influence of the nervous *Æther*, and the Influx of the *Æther* being stop'd by withdrawing the *Impetus* given to it by the Power of the Will; the Reason and Manner of their Relaxation will easily appear. For since the nervous Fluid is extremely subtle, that Portion of it which is thrown into the muscular Fibres, acts but for a Moment, or the least
Space

Space of Time, so quick is it in its Motions, and so penetrating in its Nature; and no sooner is the Vigour of the Attraction over, but the Tension of the Antagonist Muscles, and the Impulse of the Blood will extend them again.

LXXXVI.

Whoever duly considers the well known Effects of magnetical and electrical *Effluvia* will be at no Loss to conceive the instantaneous Influence which the nervous *Ether* has upon the muscular Fibres.

LXXXVII.

It must be confess'd indeed, that these *Intima Nature*, or secret Operations in the Animal Economy are all skreen'd from our Knowledge, the Agents being too subtil ever to become the Objects of our Senses, though ever so well assisted; so that we can only form our Schemes, and deduce our Arguments from such collateral Proofs, or from such *Data* as we are pretty sure are true. As for Instance; the Influence which the *Soul* has upon the *athereal Medium* in the Nerves must be by Impulse; for though our finite Capacities are not able to comprehend the Nature of immaterial Impulse; yet nothing is more certain than that the most subtil Matter in the Universe cannot be moved without some impressed Force.

LXXXVIII.

LXXXVIII.

That the Will does exert itself after this manner, is in a good measure proved by Dr. *Stuart's* * Experiment upon a Frog, where a proper Impulse being given to the *Medulla spinalis* did excite Motion in the voluntary Muscles, though the Head was sever'd from the Body.

LXXXIX.

Hence also it appears, that the Nerves are always replete with a subtil Fluid capable of contracting the Muscles, or otherwise such an Impulse on the Beginning of the Nerves, could not have excited Motion after the Head was cut off.

XC.

And again, common Experience assures us, that tho' the Nerves are always replete with an *athereal Medium*, yet this *Medium*, in a State of Health, never flies out at their Extremities, into the muscular Fibres, without some Impulse by the Direction of the Will : Whenever it happens to do so, Convulsions and Cramps are the natural Consequences.

XCI.

* Lectures on Muscular Motion.
** ;

XCI.

It may perhaps seem strange to some, that I have not all this while taken any Notice of the Blood, as an Agent in muscular Motion; since it has ever been reckoned some way necessary towards it. But notwithstanding this Opinion has been so long and so generally received, yet if our Scheme be the true one, it evidently appears the Blood hath nothing to do with the immediate Contraction of the Muscles.

XCII.

From the close Connection of the nervous *Capilla-menta* in all or most of their Ramifications, to those of the Arteries, it seems as if the *Diastole* and *Systole* of the arterial System was some how useful to them. Perhaps it may assist in pushing on the *Succus nutritius*, or that clammy balsamic Juice which is in the Nerves, towards their Extremities; but I cannot conceive that the Blood itself is in any way assisting towards muscular Motion, except it be by keeping the *Fibres* warm, supple, distended, and every way ready for the Influx of the nervous *Liber*.

XCIII.

I have tied up and cut asunder both the *Carotid* and both the *Crunal* Arteries of the same Dog, without destroying the Motion of one Muscle. Nothing less than laying a Ligature on the *Aorta descendens* will

will destroy the Motion of the hinder Parts; and possibly this may happen from the great Distension of the *Aorta* above the Ligature, pressing upon the Nerves which go to the lower Parts.

XCIV.

It is certain indeed, when all the Blood is intercepted the Fibres will soon collapse, and grow flaccid, and muscular Motion will cease, merely for Want of the Warmth, Suppleness, and Distension which the Muscles receive from the Blood. But what I think most reasonable is, that the Blood is no Way concerned as an efficient Cause in pursuing up and contracting the Fibres; it rather by its Motion through the Muscles, acts as an Antagonist to their Contraction, by extending and distending them; for the Blood, by the *Diastole* and *Systole* of the Arteries, is continually urging on its Passage through the Muscles.

XCV.

Thus I have endeavour'd to deduce and illustrate the Cause of muscular Motion from true Principles, by pursuing only those *Laws of Nature*, which our great Philosopher Sir *Isaac Newton* has in so surprising a manner discover'd to us. But I am far from thinking this a complete Account; I know it requires more Experiments, and better Reasonings than I am Master of, to explain it as it ought; and even after all, there are, and ever will be, some Things above the Reach of our Capacities to demonstrate,

strate, any otherwise than by their Effects, or second Causes. Such are, the Nature of an immaterial Impulse; the real Existence of so subtil a Fluid as is attributed to the Nerves; and the true Causes of Attraction and Repulsion.

XCVI.

That the SUPREME BEING hath implanted an immaterial Spirit in every living Creature, for the Purposes of Sensation and voluntary Motion, I think cannot be denied by any one in his Senses: But perhaps it may not become us to be too solicitous about the *Modus* of Action betwixt the *Soul* and *Matter*; these Things being above the Reach of human Reason. It is sufficient for our Purpose, that we know the Will has a Power of determining the nervous *Nether* immediately and directly to every individual voluntary Muscle.

XCVII.

The Existence or Non-Existence of the nervous Fluid, commonly called the animal Spirits, has been a Controversy of long standing. The first Searchers into the Structure of the human Body soon found that muscular Motion depended upon the Nerves, or something within them; and this has constantly been asserted, and admitted as a known Truth. The Advocates for the Existence of animal Spirits have generally supposed that voluntary Motion was performed by a sudden Inflation of the Muscles, either by the Power of the nervous Fluid itself, or by an instantaneous Ferment with some other Fluid; and I am apt to believe, that this Doctrine proving
 inconsistent

inconsistent with many Things relating to the Animal Oeconomy, and contrary to some known Experiments, might give Rise to the vibrating Scheme, where the Existence of the animal Spirits is denied, and where it is supposed that both Sensation and muscular Motion may be performed merely by the Elasticity of the Nerves, and Contractions first of all begun in the Brain, and so communicated to the fleshy Fibres: But this is so immechanical a Notion as not to deserve an Answer; it being impossible for a vibrating Motion in one Cord or String, were it ever so elastic, to cause a Contraction in another, without the Intervention of some Fluid.

XCVIII.

I shall conclude this Lecture therefore with observing, that the Existence of an *athereal Medium* in the Nerves is past all manner of Doubt; it being otherwise contrary to the known Laws of Nature for the Nerves to be the Cause of muscular Motion if they were solid, or did not admit the most subtil Fluid, secreted by the Glands of the Brain, to pass through them.

And since it is known from Experiment, that the Muscles grow less in Action, and, consequently, the constituent Particles of every Fibre must run nearer together before such a *Phænomenon* can happen, we think it very manifest that this Property of Constriction arises from the Principle of corpuscular

lar Attraction being increased and strengthened by the Influence of the nervous *Ether*; a Principle, which, from the endless Divisibility and Subtily of Matter, we may never be able to comprehend, though we know it to exist in Nature from innumerable Observations and Experiments.

The End of the Second Lecture.

LECTURE III.

XCIV.

Read April 9. ^{1747.} **I**N Confirmation of the Truth of what has been said in the two preceding Lectures, and for a further Illustration of this Subject of muscular Motion, I beg Leave to offer some Thoughts concerning the Cause and Manner of Action in the involuntary Muscles; and after that I shall relate some Experiments, which I have made in order to illustrate our Theory in general, as far as the Nature of the Subject will admit.

XCVI.

When any Muscle, voluntary or involuntary, is fully contracted, that is, when its component Particles are drawn into the closest Contacts they are capable of, by the Influence of the *athereal Medium* in the Nerves, it is evident, from all the Laws of Matter, that they would not recede from each other again without some impressed Force. Now in all the voluntary Muscles we very well know, that when one Set of them are contracted, their Antagonists are lengthen'd, and *vice versa*; so that the *Vis Resistivæ* in all the stretched-out Fibres, and the *Momentum*

Momentum of the Fluids to enter again into the contracted Muscles, will instantly dilate and distract them, when the Impulse of the Will ceases, till an exact Equilibration is restored.

XCVII.

This being the plain Matter of Fact in regard to the voluntary Muscles; let us now endeavour to find out the Mechanism belonging to the involuntary ones, and more particularly of that very curious one the Heart.

XCVIII.

Dr. Houdly, in his *Lectures on Respiration*, has manifestly shewn that the external Ranges of the intercostal Muscles appear to have all the characteristic Marks of antagonist Muscles. If you take three Ribs together, *says he*, and observe the different Ranges of the Fibres in the two intercostal Muscles, which lie on each Side the middlemost of these Ribs; you will see, at first Sight, that the internal Range in the lower Muscle, counter acts directly the external Range in the upper Muscle.

XCIX.

The Action of the *Diaphragm* is also counterbalanc'd by the abdominal Muscles, and Contents of the *Abdomen*; which squeeze it up, in a convex Form, into the Cavity of the *Thorax*, upon every Expiration,

C.

As to the *Sphincter* Muscles, they seem to be always in the same natural contractile State; and whenever they are stretched out, by some superior Power, they recover their usual Dimensions merely by their restitutive or elastic Property. The same may be said of the *Stomach*, *Uterus*, *Vesica*, which contract by the same means, into a narrower Capacity, as soon as their Contents are discharged.

CI.

I shall confine my Thoughts therefore to the Action of the Heart only, as being the most perfect involuntary Muscle.

CII.

The Heart is a *Viscus* which has given the *Literati* a great deal of Trouble to find out its real Mechanism, and the true Cause of its regular Alterations of Contraction and Dilatation. I have oftentimes laid open the Breast of a Dog, and kept his Lungs playing with a Pair of Bellows, in order to observe how regularly and alternately the *Systole* and *Diastole*, both of the Heart and its Auricles, followed ~~each other~~; that is, when the Ventricle was contracted, the Auricle was dilated, and *vice versa*; so that the Auricles seem, in some measure, to act as Antagonists to the Ventricle, and the resultant Blood may be of the same Service to the Auricles.

icles. But there being so much Disparity between the contractile Strength of the Ventracles and that of the Auricles, there must necessarily be some other Cause, which, when the Heart is fully contracted, makes it unbend again, or cease to contract; by which means the Auricles, though so very weak in comparison of the Ventracles, have Power enough to throw in Blood, and thereby to distend the Ventracles to a certain Degree, before they are capable of acting again.

CIII.

If we might be allowed to make an Estimate of the Difference between the contractile Strength of the Heart and its Auricles, from the Difference of their Bulk, or Quantity of Fibres; we should find it to be about *nine to one*: as I have observed, by weighing the Hearts of several Animals, and their Auricles separately.

CIV.

Hence it does not seem reasonable to think, that the weak Efforts of the Auricles would be sufficient to cause the *Diastole* of the Heart, without some other Assistant.

CV.

One great Use of the Auricles is, to receive a Quantity of Blood, during the *Systole* of the Heart; sufficient to fill the Ventracles again at their *Diastole*.

stole. The same may be said, in some measure, of the Veins nearest the Heart, which may be plainly seen to dilate during the Contraction of the Auricles; so that the Heart, Auricles, and Veins, have all their *Systoles*, and *Diastoles* in subordinate Degrees.

Without such Receptacles as these it would be impossible for the Ventricles of the Heart to be fill'd from the Veins so suddenly as they are; for though the *Areas* of the transverse Sections of the Veins are much larger than those of the Arteries, yet we find, by Dr. Hales's *Haematological Experiments*, that the Velocity of the Blood in the Arteries is above six to one to that in the Veins: But since the Auricles keep receiving the Blood whilst the Heart is in its *Systole* (the Veins doing the same by the Auricles) a due Quantity of Blood is always ready to be thrown into the Heart, by the Time it is fit to receive it; for the *Diastole* takes up two Thirds of the Space of Time between each Pulse.

Here then we may observe that the *Impetus* of the reflux Blood acts upon, and distends the Veins nearest the Heart, whilst the Auricles are contracted; that it also dilates the Auricles, whilst the Heart is in Action; and that the Heart is in its *Diastole*, whilst the Auricles are in their *Systole*.

But the great Difficulty in accounting for the *Dia-
stole* of the Heart, is from the Disproportion between
its contractile Power, and that of the Auricles; it
being plain, from what has been said above, that
the *Momentum* of the Blood in the Veins is stopt by
the Contraction of the Auricles, so that the Ventricle
of the Heart can receive no other Impulse from
the Blood at that time, than what is derived from
the Contraction of the Auricles.

CVII.

Hence therefore it follows, that if the *Systole* was
the natural State of the Heart, and to which it al-
ways tended with its full contractive Power, the
Impulse of the Blood, from the Contraction of the
Auricles, could never be able to dilate it.

CIX.

Dr. *Lower* makes the *Systole* the natural State or
Action of the Heart, and the *Dia-
stole* the violent
one: *Boerhaave*, on the contrary, makes the *Systole*
the violent, and the *Dia-
stole* the natural State. But
perhaps neither of these Opinions may be right, in
the strictest Sense; for if we look back we shall
find, that if the Fibres were not tense they could
not be elastic; and if some Violence was not put
upon them, by the Impulse of the circulating Fluids,
they would not be tense: Hence it appears, that
Elasticity proceeds from a State of Violence.

CX.

And again; When a Muscle is freed from the Power of its Antagonist, and is thereby left at full Liberty to contract, as it always will by its elastic, restitutive Property only, it does so no further than in Obedience to the common Power of Attraction between its component Particles; but in all other muscular Action, when this attractive Power is greatly increased by the Influx of the nervous *Æther*, the Constriction is carried much further, and the Muscle is more fully contracted than it ever is in the other State: Hence it follows, that Contraction, in its fullest Degree, is not the natural State of a Muscle.

CXI.

And further; When the constituent Particles of the Fibres are drawn into their closest Contacts, by the Influx of the nervous *Æther*, it requires some Force, in a contrary Direction, to elongate the Fibres again; so that Extension, or Dilatation is also a State of Violence.

CXII.

From the Whole then it appears, that neither the *Systole* nor *Diaſtole*, in a full Degree, is the natural State of the Heart; and this we shall shew more plainly hereafter by inspecting the Hearts of Animals after being bled to Death. But to proceed:

CXIII.

CXIII.

From what has been said above, it seems reasonable to conclude, that if Contraction, in its fullest Degree, was the natural State of Rest or Quiescence in the Heart; the *Momentum* of the Blood from the Contraction of the Auricles, could not be a sufficient Counterpoise. And since so many fruitless Attempts have been made to account for the *Diastole* of the Heart; from the *Impetus* of the Blood in the Veins, and from the Pressure of the Atmosphere, &c. give me Leave to propose the following *Queries*.

CXIV.

1st. May not the Heart be a compound Muscle; that is, may it not have its Antagonist within itself? Or, in other Words, are not some of its Fibres so ranged, that whilst one Set of them is contracted and shorten'd, others may be stretched out; analogous to the Action of the intercostal Muscles, or any other Muscles with their Antagonists?

CXV.

2^{dly}. Is the nervous *Æther* transmitted from the Brain to the Heart in a pulsatory Manner, at equal Distances of Time; or may it be supposed to move uniformly through the Nerves, and some Interruption is given to its Influx into the muscular Fibres, when the Heart is in its *Systole*?

CXVII.

3dly. Does not the *Diaſtole* of the Heart depend upon an Abatement of the Tension in the contracted Fibres; a Motion of Reſtitution in ſuch as are over ſtretched; and the Influx of the Blood conjunctly?

CXVIII.

Without ſome ſuch Mechanifm as this, no Power that we know of, belonging to the animal Oeconomy, would be able to cauſe the *Diaſtole* of the Heart: But if ſuch a Structure, as is above-mention'd, could be proved, no more Difficulty would attend the Explication of it than that of the voluntary Muſcles.

CXIX.

In diſſecting the Heart we find a great many different Orders or Series of Fibres, variously contorted, and running in contrary Directions; ſo that, for ought we know, it may be an *Epitome* of muſcular Conſtruction in general.

CXX.

We may here obſerve what Care *Nature* has taken to prevent too great a Dilatation in the *Diaſtole* of the Ventricles, the right one eſpecially, as being the weak-eſt, by forming the *Papillæ*, or *Columnæ*, which run
from

from the *Septum*, or middle Partition, to its opposite Sides; whereby they act as so many Braces in the *Diaſtole*, and when they contract, they alſo aſſiſt in the *Syſtole*. And perhaps it may not be the moſt improbable Conjecture to think; that as much Care may have been taken in providing a ſufficient Number of Fibres, or little Muſcles, which may be ſo formed as to act in the *Diaſtole*, as Antagoniſts to thoſe which occaſion the *Syſtole*.

CXXI.

The Hearts of Frogs, Vipers, Eels, &c. ſeem to evince the Reaſonableneſs of this Conjecture, by continuing their *Syſtoles* and *Diaſtoles* after they are taken out of the Body; when there is no reſluent Blood to dilate the Ventracles, and, conſequently, if there were no Fibres upon the Stretch, when the Heart is contracted, which, by their Elafiicity, or reſtitutive Power, did pull back, or elongate ſuch as were contracted, there could not poſſibly be any *Diaſtole* in ſuch Circumſtances.

CXXII.

We are aſſured by Mr. *Boyle*, in his *Phyſico-Mechanical Experiments*, that the Heart of an Eel hath continued to beat an Hour, in an exhausted Receiver; after which, finding its Motion very languid, and almoſt ceaſed, by breathing a little upon that Part of the Glaſs where the Heart was, it quickly regain'd Motion; and an Hour after that,
finding

finding it almost quite gone, he was able to renew it, by the Application of a little more Warmth.

CXXIII.

The same *illustrious Author* further assures us, that he has sometimes cut the Heart of a Flounder transversely, into two Parts; and freeing each from the Blood it contain'd, he observed, for a considerable Time, that both of them together continued their former Contraction and Relaxation. And once, thus cutting one into several Pieces, he found, to his Surprise, that they not only moved as before, but that even the Whole, thus separated, long preserved the same Succession of Motion, as appeared therein whilst coherent.

CXXIV.

Now can the *Diaſtole* of the Heart, in these Experiments, be accounted for upon any other Principles than those we have laid down? Here was no Impulse from the reflux Blood to dilate the Auricles, or to distend the Ventricles. The Pressure of the Atmosphere was also quite taken away in the first Experiment, and could not possibly be of any Service in the last; and yet the *Diaſtole* continued.

CXXV.

Can the same Fibres which are contracted, have it in their own Power to fly out again to their usual Lengths? If this cannot be, what Power is there

there in a Heart, taken out of the Body, to unbend itself, or to dilate its Ventricles, after they are once contracted, unless we suppose some of its Fibres to act as Antagonists to others?

CXXVI.

We have good Reason therefore to believe that some Fibres of the Heart are always stretched out beyond their natural Tone, when others are contracted; so that, by their elastic, restitutive Property, they do act as Antagonists in a certain Degree.

CXXVII.

In regard to the *Systole*, in such Hearts as are taken out of the Body, and cut into several Pieces, we conceive, that as long as Warmth and Moisture remain, so long may the *æthereal Matter* in the Nerves, continue to fly into the Fibres, and contract them; and when it ceases, more Warmth, or a gentle Impulse (even with the Point of a Needle only) will revive the Motion.

CXXVIII.

Hence we may, in some measure, discover the amazing Subtilty of the nervous *Æther*; when such very small Sections of the Nerves, as in the above-mention'd Experiment upon the Heart of a Flounder, should contain Matter sufficient for so many Contractions.

CXXIX.

CXXIX.

Let us now return to our *second Query*, and examine whether the nervous *Æther* is transmitted from the Brain to the Heart, in a pulsatory Manner, at equal Distances of Time; or whether some Interruption is only given to its Influx into the muscular Fibres, when the Heart is in its *Systole*.

CXXX.

In order to understand this, we must look back and consider, that, according to our Theory, if the *æthereal Medium* in the Nerves was perpetually flying into the muscular Fibres of the Heart, it would be constantly contracted, notwithstanding the *Momentum* of the Blood, the Contraction of the Auricles, or the *Vis Restitutionis* in the stretched-out Fibres. Hence then it is evident, that the alternate Contractions and Dilatations of the Heart proceed from an alternate Influence of the nervous *Æther*; but how this Alternation happens, when the Nerves which supply the Heart are not, in the least, under the Direction of the Will, is the Difficulty we labour under.

CXXXI.

In all the Nerves which supply the voluntary Muscles, it is certain there are Restrictions in some Parts of them which the *æthereal* Matter is not able to dilate without an additional Impulse from the Will;

Will; or otherwise Cramps and Convulsions would perpetually happen. But in those which supply the Heart, the Motion of the *Æther* through them cannot have any Assistance or Impulse from the Will, as not being in the least under its Influence; so that the Vibrations of the *Meninges* of the Brain, and the Dilatations of the Arteries may be supposed to be the Agents which propel the *Æther* towards the Heart.

CXXXII.

Hence then it seems to follow, that the alternate Contractions of the Heart may proceed from the alternate Impressions made on the Nerves, by the *Meninges* of the Brain, and Dilatations of the Arteries.

CXXXIII

Upon opening the Skulls of living Animals, the *Dura Mater* may plainly be seen to have its *Systoles* and *Diafoles* corresponding to those of the Arteries; but since the Heart continues to beat after the Head is cut off, or even after it is taken out of the Body, where there cannot be any alternate Succussions made on the Nerves by the *Meninges* of the Brain, or by the Pulsation of the Arteries, it seems reasonable to believe that this Alternation is occasion'd by some Impediment being given to any further Influx of the *Æther* into the muscular Fibres of the Heart, when it is fully contracted; or otherwise, the Heart would constantly remain in a

State of Contraction, as long as there was any *æthereal Matter* flying from the Nerves.

CXXXIV.

If we consider in how many different Directions the muscular Fibres of the Heart run, how much they are corrugated, thickened, and swelled, when fully contracted, and how strong and uniform the Pressure must be in their greatest Degree of Action; it may not perhaps appear unreasonable to think that the Extremities of the Nerves, which are inserted into every Fibre, and which are extremely small and tender, may be pressed upon and squeezed, so as to prevent the Influx of the *Æther*, till the Pressure is abated, or till the Fibres are extended again to their usual Lengths.

CXXXV.

What seems to evince the Reasonableness of this Supposition is the Nature of the *Shaking Palsy*; where the voluntary Muscles immediately become involuntary ones, so far at least, as to be alternately contracted and relaxed without the Consent or Direction of the Mind.

CXXXVI.

Now this Distemper we believe to proceed from a particular Weakness in the Nerves, whereby those little Restrictions in them, which keep the *æthereal Matter* within due Bounds, in a State of Health,
are

are so far weaken'd and destroyed, that the *Æther* has a Power of flying into the muscular Fibres without any Impulse or Direction from the Will, after the same manner it does into the Heart. If therefore no Interruption was to be given to the Influx of the nervous *Æther* by the Muscles themselves, when they were contracted, it would follow, that if the *Flexors* of any Limb were to be first contracted, the *Extensors* would not be able to recover the *Equilibrium*, and to be contracted in their Turn, without the Assistance of the Will; for if our Doctrine be right, *viz.* that muscular Motion proceeds from the constituent Particles of the Fibres being drawn into closer Contacts by the attractive Influence of the nervous *Æther*, it necessarily follows, that if the same Quantity of *Æther* was continually to fly into a Muscle already contracted, it would have a greater Influence on the component Particles so approximated, than on others in the antagonist Muscles, which are distracted, and consequently touch each other in fewer Points.

CXXXVII.

Hence it seems to be evident that some Impediment is given to the Influx of the nervous *Æther* when the Muscles are contracted, or otherwise that they would always remain so; for, though the Will may be able to remove such little Impediments, and to keep the voluntary Muscles in a State of Contraction for a considerable Time; yet since the voluntary Muscles, when affected with a Palsy, are regularly and alternately contracted, and have their

Systoles and *Diaстоles* analogous to the Heart and its Auricles, I think we have good Reason to conclude, that the same Principles which contract and dilate the voluntary Muscles, in the above-mention'd Condition, are the Principles which occasion the *Systole* and *Diaстоle* of the Heart, with this Difference only, that the Influx of the Blood into the *Sinuses* of the Heart, is what no other Muscle in the Body has or receives; and without Doubt this may truly be reckon'd the greatest Assistant in dilating the Ventricles of the Heart, after the attractive Influence of the nervous *Æther* ceases, and the *Æquilibrium* is restored between the stretched-out Fibres and those which were contracted.

CXXXVIII.

What has been said seems greatly to evince the Truth of our *third* and *last* Supposition; viz. that the *Diaстоle* of the Heart may depend on an Abatement of Tension in the contracted Fibres, a *Vis Restitutionis* in such as are over-stretched, and the Influx of the Blood conjunctly.

CXXXIX.

Give me Leave to relate two or three Experiments which I have made, in order to illustrate the foregoing Theory, and then I shall conclude.

CXLI.

EXPERIMENT I.

Having observed that the Ventricles of the Hearts of all Animals, when bled to Death, are dilated, or stand wide open to a certain Size, though there happens to be very little Blood in them, it seems as if the *Diaſtole*, in a certain Degree, was the laſt Motion of the Heart. In order to know the Truth of this, I open'd the *Thorax* of a Dog, and kept his Lungs playing with a Pair of Bellows, that I might perfectly ſee the ſeveral Actions of the Heart and its Auricles.

CXLII.

The Auricles ſeem'd to me to begin the Motion, and the *Syſtole* of the Heart always inſtantly followed that of the Auricles. Then the *Apex* and Sides of the Heart ſunk down, and were lengthen'd of their own accord, before any Blood was thrown into the Ventricles, from the Contraction of the Auricles; for there was oftentimes more than double the Time taken up in the *Diaſtole* of the Heart, than the *Syſtole* both of the Auricles and Ventricles required.

CXLIII.

If I deſiſted from blowing freſh Air into the Lungs for ſome little Time, the Heart would lie ſtill, and yet I could recover its Motion again, by
ſtrongly

strongly distending the Lungs. In this Action I never could discern that the Heart began the Motion, but the Auricles always contracted first, and then the Heart immediately afterwards; though, at last, I saw several Contractions of the Auricles, which were not succeeded by any Motion of the Heart.

CXLIV.

I must not forget to mention, that in another Dog I saw several *Systoles* and *Diaستoles* of the Heart after I had purposely cut asunder both the *Vena Cava's*; and could plainly perceive the Ventricles to relax and open themselves, when very little or no Blood could possibly flow into them. This however is much more easily seen in the Heart of a Viper, when taken out of the Body, which will continue its *Systole* and *Diaستole* for a long time; and if it ceases to beat ever so often, and you renew its Motion again and again, by breathing upon it, &c. You may observe that the Auricle always contracts before the Ventricle, and that the *Diaستole* is the last Motion, though there is no Blood to distend the Heart.

CXLV.

The Capacity of the right Ventricle of the Dog's Heart, in the above Experiment, as it open'd of its own accord, was something more than a cubic Inch, as I found, by pouring in melted Wax from a Ladle, without any Pressure from a perpendicular Height,

Height, after the Auricle and Blood-vessels were cut off transversely.

CXLVI.

EXPERIMENT II.

Taking the Heart out of an Ox as soon as I possibly could after he was kill'd, and having cleansed it from the Blood, by washing it in warm Water, I fill'd both the Ventricles with melted Wax, without any distending Force more than by pouring it from the Ladle. The Heat of the Wax seem'd rather to contract the Fibres, and to lessen the Capacities of the Ventricles; for some of the Wax kept running out from the Heart after I had done pouring it from the Ladle.

CXLVII.

The Capacity of the right Ventricle was equal to $5\frac{1}{2}$ cubic Inches. The Capacity of the left Ventricle was not quite 5 cubic Inches.

CXLVIII.

From these Experiments it manifestly appears, that the last or restitutive Motion of the Heart is to dilate or open the Ventricles, and that without any Assistance or Impulse from the reflux Blood.

CXLIX.

CXLIX.

Dr. *Hales* * injected the left Ventricle of the Heart of an Ox from a Column of melted Wax $4\frac{1}{2}$ Feet high, which distended the Capacity of it to 12. 5. cubic Inches. Now supposing the Impulse of the reflux Blood to be equal to a Column of melted Wax $4\frac{1}{2}$ Feet high, and that in every *Dia-stole*, whilst the Ox was alive, the left Ventricle was distended to the Capacity of 12. 5. cubic Inches, yet we may observe that the Heart, immediately after each *Systole*, relaxes and dilates its Ventricles to a certain Degree, by its own Mechanism, and to whatever Capacity the Ventricles are afterwards dilated, it must necessarily proceed from the Impulse of the Blood only; all which is perfectly agreeable to our *third Query*, viz. that the *Dia-stole* of the Heart may depend upon an Abatement of the Tension in the contracted Fibres, a Motion of Restitution in such as are over-stretched, and the Influx of the Blood conjunctly.

CL.

EXPERIMENT III.

I took the Heart out of a live Viper, and placing it upon a Piece of writing Paper, I found it beat
at

* *Hæmastatics*, pag. 25.

at the rate of 24 or 25 Pulsations in a Minute, for three or four Minutes. After this I laid the Paper upon the Palm of my Hand, the Warmth of which increased the Number of Pulses to 37 the first Minute, and to 48 the second. Last of all I put it into warm Water, a Degree or two warmer than the Blood, where it beat 87 Pulses the first Minute, and afterwards declined in its Motion more and more till it quite ceased.

CLI.

May we not from hence observe what vast Influence *Heat* had upon the nervous *Æther* to make it fly so quick into the Heart as to increase the Pulse to almost four times their usual Number? How far the Heat in some sort of Fevers may quicken the Pulse, is not to our present Purpose to enquire.

CLII.

It is amazing to think that such very small Portions of the Nerves, as were cut out with the Heart in this Experiment, should contain Matter sufficient for so many Contractions; and that it did not all of it instantly fly out from the wounded Ends, even before it was put into warm Water.

CLIII.

How far these Experiments may serve to illustrate and confirm the Truth of our Theory of muscular Motion, is very humbly submitted to the candid
I Judgment

Judgment of this *Honourable* and most *Learnea Society*; and I shall think myself well recompensed for my Pains, if it so far meets with their Approbation as not to contain any thing inconsistent with true Philosophy, or to any of the known Laws of the Animal Oeconomy.

W. R. G.

F I N I S

